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CONTENTS

ON THE AUTHORSHIP OF THE ANONYMOUS PAMPHLET PUBLISHED IN LONDON IN 1760 ENTITLED "THE INTEREST OF GREAT BRITAIN CONSIDERED WITH REGARD TO HER COLONIES AND THE ACQUISITION OF CANADA AND GUADALOUPE."

By I. MINIS HAYS.

(*Read April 24, 1924.*)

The Seven Years' War—the conflict maintained by Frederick the Great of Prussia against Austria, Russia and France—began in 1756; England allied herself with Prussia and undertook to curb the power of France in America and especially in Canada, where her arms met with marked success. In July, 1758, Louisburg, with its strong fortress commanding the mouth of the St. Lawrence, was captured by General Amherst; in September of the following year Quebec fell to Gen. Wolfe and the War in this country was ended in September, 1760, by the surrender at Montreal of the French Army in Canada.

In England the terms of the coming peace had been actively discussed for some time previous thereto. Two anonymous pamphlets on this subject, which appeared in the beginning of 1760, attracted marked attention. The first was entitled "A Letter Addressed to Two Great Men on the Prospect of Peace; and on the Terms Necessary to be Insisted Upon in the Negotiation" (London, 1760), which is supposed to have been inspired by William Pulteney, Earl of Bath, and to have been written by John Douglas, afterward Bishop of Salisbury; and the two great men to whom it was addressed were William Pitt, Earl of Chatham, and the Duke of Newcastle, both influential members of the Government. Among other points it strongly urged were the acquisition by Great Britain of Canada rather than territory in the West Indies.

This pamphlet was promptly answered by another entitled "Remarks on the Letter Addressed to Two Great Men" (London, 1760),

which ran through three editions in Great Britain, and was reprinted in Boston in the same year. In it opposite views were urged and the retention of Guadaloupe, captured by the English in 1759, was strongly advocated, and it was claimed that its sugar trade was more important to Great Britain than was the fur trade of Canada. It was probably written by William Burke, the supposed author of the "Letters of Junius," but it has also been ascribed to Rt. Hon. Charles Townshend.

These anonymous pamphlets were promptly followed by another, entitled "The Interest of Great Britain Considered with Regard to Her Colonies and the Acquisition of Canada and Guadaloupe" (London, 1760), which immediately attracted marked attention and was reprinted in the same year in Dublin, in Philadelphia and in Boston in two editions, and a second edition was issued in the following year in London, so great and widespread was the interest it excited. It summarized the arguments of both the preceding pamphlets and strongly supported the views advanced by Douglas. The authorship of this pamphlet has been attributed by some bibliographers to Franklin and by others to Richard Jackson.

The American colonies were becoming alarmed by the proposals concerning the restoration to France of Canada and the Island of Guadaloupe and they felt that the outcome was of vital interest to them. There was no abler advocate for the retention of Canada than Dr. Franklin, who was then in London, whither he had been sent in 1757 by the Assembly of Pennsylvania to secure its right to tax the estates of the Proprietaries in common with the other lands in the Colony. In this, his first diplomatic mission, he had been eminently successful and thereby acquired considerable prestige. He remained abroad until 1762 and in all matters relating to the American Colonies his opinion was eagerly sought.

That he was deeply interested in the terms of the coming Treaty and was endeavoring to influence British opinion in relation thereto is shown by his letter to John Hughes, of Pennsylvania, under date of London, January 7, 1760, in which he says:

"There has been for some time a Talk of Peace, and probably we should have had one this winter, if the King of Prussia's late

Misfortunes had not given the Enemy fresh Spirits, and encourag'd them to try their Luck another Campaign and exert all their remaining strength that if possible they might treat with Hanover in their Hands. If this should be the Case, possibly most of our Advantages may be given up again at the Treaty, and some among our great Men begin already to prepare the Minds of People for this, by discoursing, that to keep Canada would draw on us the Envy of other Powers, and occasion a Confederacy against us; that the Country is too large for us to people, not worth possessing, and the like. These Notions I am every day and every where combating, and I think not without some Success. The Event God only knows. The Argument that seems to have a principal Weight is, that in Case of another War, if we keep Possession of Canada, the Nation will save two or three Millions a Year, now spent in Defending the American Colonies, and be so much the stronger in Europe, by the Addition of the Troops now employ'd on that side of the Water. To this I add, that The Colonies would thrive and increase in a much greater Degree, and a vast additional Demand arise for British Manufacturers, to supply so great an Extent of Indian Country, etc., with many other Topics, which I urge occasionally, according to the Company I happen into, or the Person I address." (Franklin Mss. in American Philosophical Society, XLV., 19*a*).

Richard Jackson, to whom the authorship of this last pamphlet has also been and is still not infrequently attributed, was an English lawyer interested in politics and from his extraordinary stores of knowledge was commonly known as "Omniscient Jackson." At the time of its appearance he was Agent in London for the Colony of Pennsylvania and Franklin rated him as one of the best authorities "for everything related to America" (Smyth, *loc. cit.*, V., 67). So learned an authority as Hildeburn says in his "Issues of the Pennsylvania Press" (Philadelphia, 1885, I., 350) that this pamphlet was "commonly attributed to Franklin when first published, but is now known to have been written at his instance by Richard Jackson," but he does not give his proof for this statement. Sabin ("Dictionary of Books Relating to America," New York, 1877, IX., 184) and Cushing ("Anonyms," Cambridge, 1890) follow him. On the other hand, Evans ("American Bibliography," Chicago, 1905, III., 254), Halkett and Laing ("Dictionary of Anonymous and Pseudonymous Literature of Great Britain," Edinburgh, 1885, II., 1241) and Rich ("Bibliotheca Americana Nova," London, 1835, I., 133) ascribe it to

Dr. Franklin. In the Catalogue of one of our largest historical libraries the writer has found the author-entry for this pamphlet under Jackson and no reference whatsoever to it under Franklin.

After diligent search and with every desire to do justice to Richard Jackson, the facts upon which Hildeburn based his conclusion have not been found—indeed, the available data regarding Jackson are very meagre. "The Encyclopedia Britannica" does not mention him, and the "Dictionary of National Biography" (London, 1892, XXIX., 104) accords to him less than a half page and makes no reference to his having been in any way connected with the authorship of "The Interest of Great Britain Considered."

Contemporary writers with considerable unanimity ascribed the authorship to Dr. Franklin. Chief Justice Edward Shippen in a letter, quoted by Hildeburn, dated October 2, 1760, says: "I have seen B. Franklin's Pamphlet, and think it the best book that has been published in England concerning American Affairs. He appears to have thoroughly understood his subject and has done justice to the cause he espouses. Certainly we should be dupes to give up Canada" (*loc. cit.*, I., 350). In a letter to Lord Kames, under date of London, May 3, 1760, Dr. Franklin says: "I have endeavoured to comply with your request in writing something on the present situation of our affairs in America, in order to give more correct notions of the British interest with regard to the colonies, than those I found many sensible men possessed of. Inclosed you have the production, such as it is. I wish it may in any degree be of service to the public. I shall at least hope this from it, for my own part, that you will consider it as a letter from me to you, and take its length as some excuse for its being so long a-coming" ("Memoirs of Hon. Henry Home of Kames," Edinburgh, 1807, I., 268, consult also Franklin's letter to Lord Kames, of Jan. 3, 1760, Smyth, *loc. cit.*, IV., 4). The enclosure was clearly a printed copy of the pamphlet in question.

Dr. Franklin evidently sent copies of this pamphlet to some of his friends. William Thomson, in a letter to him from Worcester, England, under date of November 18, 1760, says: "I take this Opportunity to return you my sincere thanks not only for the pleasure of your Company afforded me during your short stay in Worcester,

but also for the Entertainment I am confident I owe to you after your Departure—*The Interest of Great Britain with Respect to Her Colonies* gave me a more distinct view than I ever had before of our Connexions w[t] our fellow Subjects in distant parts of the Globe." (Franklin Papers in Amer. Philos. Soc., I., 60). The conclusion to be drawn from this note is that, in evidence of appreciation of his hospitality, Dr. Franklin sent to Mr. Thomson a copy of his pamphlet, and had it not been written by him he, at the time, certainly would have disclaimed its authorship which under the circumstances Thomson would be sure to attribute to him.

Benjamin Vaughan, who was an intimate friend of Franklin and in frequent correspondence with him, in his edition of Franklin's "Political, Miscellaneous and Philosophical Pieces" (London, 1779) included this pamphlet, and in an "Addenda" stated that "Dr. Franklin has often been heard to say that in writing this pamphlet he received considerable assistance from a learned friend who was not willing to be named." Franklin, under date of Passy, November 9, 1779 (Smyth, *loc. cit.,* VII., 410), thanked Vaughan "for the great Care and Pains" he had taken in preparing this edition of his writings and says that he has "noted some Faults of Impression that hurt the Sense, and some other little Matters which you will find all in a Sheet under the title of *Errata,*" but he in no way disclaimed the authorship of this pamphlet which had been set down to him. In the Italian translation of Vaughan's Collection, which appeared four years later, this essay was still included (Padua, 1783).

Paul Ford in his "Franklin Bibliography" (Brooklyn, 1889, page 117) states that Francis Maseres wrote to Vaughan claiming that nearly two thirds of the pamphlet was written by Jackson, but failed to satisfy him as to the accuracy of the statement.

Appended to the pamphlet on "The Interest of Great Britain" is an essay entitled "Observations Concerning the Increase of Mankind, Peopling of Countries, etc.," which reinforces the argument in reply to the contention that "if we had Canada we could not people it without draining Britain of its inhabitants."

In 1754, Dr. William Clarke, of Boston, was corresponding with Dr. Franklin in regard to measures for the protection of the English

Colonies against the encroachments of the French (Massachusetts Historical Society, Collections, IV., 74), and in the following year Clarke published an anonymous pamphlet entitled "Observations on the late and present Conduct of the French, with Regard to their Encroachments upon the British Colonies in North America. To which is added, wrote by Another Hand, Observations Concerning the Increase of Mankind, Peopling of Countries, &c." (Boston, 1755, and reprinted in London in the same year). The added essay is universally attributed to Dr. Franklin and is said to have been written by him as early as 1751. It seems most natural then for him, if he were the author of this new pamphlet entitled the "Interest of Great Britain considered with Regard to her Colonies," to have appended his former pamphlet in support of one of his main arguments and with the following explanatory introduction: "In Confirmation of the Writer's Opinion concerning *Population, Manufactures, &c.* he has thought it not amiss to add an Extract from a Piece written some Years since in *America*, where the Facts must be well known, on which the Reasonings are founded."

In the Boston reprint of "The Interest of Great Britain Considered" the following was added to the title page: "As the very ingenious, useful, and worthy Author of this Pamphlet [*B......n F......n*, LL.D.] is well known and much esteemed by the principal Gentlemen in *England* and *America*; and seeing that his other Works have been received with universal Applause; the present Production needs no further Recommendation to a generous, a free, an intelligent and publick-spirited People." This edition was printed and published by Franklin's nephew, Benjamin Mecom, who had been an apprentice in his Uncle's printing office in Philadelphia, and it cannot be doubted that he sent a copy of his reprint with its fulsome and conspicuous ascription of authorship to his Uncle whom he so much admired, and that the authorship was not repudiated by Franklin is evidenced by its reappearance on the title page of Mecom's second Boston edition.

William Temple Franklin, Sparks, Bigelow and Smyth, the editors of the various standard editions of Franklin's complete works, have included this pamphlet and thereby given to it their endorsement of

his authorship, as had Almon in his Biographical, Literary and Political Anecdotes (London, 1797, II., 199). Then, too, in several copies of the pamphlet which have come under the writer's notice their original owners have taken the pains to write on the title page that the author was Dr. Franklin.

Temple Franklin's version is that his grandfather "stepped into the controversy" and wrote a pamphlet, in which he was assisted by his friend Mr. Richard Jackson (who desired not to be known in the occasion)" ("Memoirs of the Life and Writings of Benjamin Franklin," 2d ed., London, 1818, I., 307). Parton also ascribes it to Franklin and says: "Tradition reports that it influenced the Ministry in deciding to keep Canada. Some have gone so far as to say that England owes that inestimable possession to Franklin, who first advocated its conquest, and then urged its retention" ("Life and Times of Benjamin Franklin," Boston, 1867, I., 422).

But on the point of the authorship there is still more direct and conclusive proof. Franklin before writing his "Autobiography" made an outline draft which was preserved and is printed in Bigelow's edition of that work (Philadelphia, 1868) and in this outline appears the following entry: "I am sent to England. Negotiations there. *Canada delenda est.* My Pamphlet. Its reception and effect. Projects drawn from me concerning the Conquest" (p. 63). Unfortunately the "Autobiography" was never written up to this date and therefore does not mention this controversy in which Franklin took such an active part, but the words "*my* pamphlet," in his skeleton outline, can refer to no other than the one under consideration. Moreover, Franklin was not a man to accept silently an oft-repeated attribution of authorship to which he was not entitled, and his silence can only be regarded as admission of its correctness.

With all these facts available it is difficult to understand why Smyth—the latest editor of Franklin's collected works, should have "found the problem of its authorship so difficult, and the question of the relative shares of Franklin and Jackson so intricate, that I am quite unable to unloose its Gordian knot" (*loc. cit.*, I., xii).

For the further elucidation of this problem, however, there are fortunately preserved in the Franklin Papers in the American Philo-

sophical Society (L., pt. ii, 13) six manuscript pages of this pamphlet, viz., folios 7, 43, 45, 53, 58 and 67, wholly written in Franklin's well-known large, clear hand with his alterations and erasures to which the printed text conforms. The pages measure 12¾ by 8 inches and some of them are considerably stained and mildewed and not without holes. Their condition and the fewness of their number may be explained by the fact that just before the occupation of Philadelphia by the British troops in 1777, Dr. Franklin's papers were boxed and sent for safe-keeping to the country place of his friend Joseph Galloway at Trevose, near Bristol, in Pennsylvania. During the military operations about Philadelphia the British troops visited Mr. Galloway's house, broke open the boxes containing these papers and many of them were scattered over the ground and trodden under foot, and it may be that some of them were carried off, certainly many of them were lost or destroyed. After the evacuation of this territory by the British forces Dr. Franklin's son-in-law, Richard Bache, repaired to Trevose, collected the scattered, mud-bespattered and otherwise injured remains of the contents of these boxes and returned them to Philadelphia. This episode accounts for the imperfect and dilapidated condition of some of the Franklin papers of which these sheets form a part.

A count of the number of written words, say, on folio 53 of Franklin's manuscript and the number of words in a page of the printed pamphlet shows that fifty-three times the number of words in the manuscript page would make a little over 33 pages of the printed text, and it is on page 33 of the printed pamphlet that the text of ms. folio 53 appears. Therefore the preceding manuscript pages must have contained about the same number of words to a line and of lines to a page, indicating that they were almost certainly written by the same hand.

It might be contended that Jackson had written part of the pamphlet and given his manuscript to Dr. Franklin, who had then added it to or copied it into his own. But this is most unlikely, if not inconceivable, in the case of a man so busy, so indisposed to unnecessary labor and so saving of his time. Then, too, Jackson's handwriting was as characteristic in its way as Franklin's, but not so free and

open, and would average more words to the page and not fit in with the calculation just made.

From whatever point of view we look at it, there can be no reasonable doubt that the " Interest of Great Britain considered with Regard to her Colonies " was written wholly by Dr. Franklin, although before committing his views to writing it was quite natural for him, as Temple Franklin states he did, to have discussed them with Jackson, his friend and fellow agent of Pennsylvania, and to have consulted him as to the plan and scope of the pamphlet he was about to write to influence British opinion in its own interest and that of the American Colonies. That Jackson wrote any part of this pamphlet there is no evidence that we have been able to discover and the original manuscript pages of it which have been preserved in the Franklin Manuscripts in the American Philosophical Society appear to refute the claim.

Peace was concluded by the signing of the Treaty of Paris on February 10, 1763, by which Canada and that portion of Louisiana between the Allegheny Mountains and the Mississippi River were ceded to Great Britain, and Guadaloupe was returned to France—thus constituting Franklin's second diplomatic success in his first mission to England.

PHILADELPHIA,
April 17, 1924.

THE CURVE OF POPULATION GROWTH.[1]

By RAYMOND PEARL.

In a series of papers from the writer's laboratory[2] it has been shown that one or another of the special forms of the general curve

$$y = \frac{k}{1 + me^{-a_1x - a_2x^2 + a_3x^3 + \cdots a_nx^n}} \qquad \text{(i)}$$

describes with extraordinary accuracy the growth of human populations and of populations of lower organisms grown under experimentally controlled laboratory conditions.

The curve was derived from the following postulates:

1. If any discussion of the growth of human population is to be profitable in any real or practical sense, the *area* upon which the population grows must be taken as a finite one with definite limits, however large. For the growth of human populations the upper limit of finite areas possible of consideration, must plainly be the habitable area of the earth. Smaller areas, as politically defined countries, may be treated each by itself. But whether this it done or not, there clearly is an upper finite limit of area on which human population can grow.

2. If there is a finite upper limit to the area upon which population may grow, then with equal clearness there must be a *finite upper limit to population itself,* or in other words to the number of persons who can live upon that area. It is obvious for example that it is a biological impossibility for so many as 50,000 human beings to live, and derive support for living, upon one acre of ground, provided every other acre of the possibly habitable area of the earth is at the same time inhabitated to the same degree of density. This

[1] Papers from the Department of Biometry and Vital Statistics, School of Hygiene and Public Health, Johns Hopkins University. No. 101.

[2] Pearl, R, and Reed, L.J., *Proc. Nat. Acad. Sci.,* Vol. 6, pp. 275–288, 1920; Vol. 8, pp. 365–368, 1922. *Id,* "Predicted Growth of Population in New York and its Environs." New York ("Plan of New York and its Environs"), 1923, p. 42. *Id, Metron,* Vol 3, pp. 6–19, 1923. Pearl, R., "Studies in Human Biology, Baltimore" (Williams & Wilkins Co.), 1924. (In press.)

is obviously true whatever the future may hold in store for us in the way of agricultural discoveries, improvements, or advancements. That there is a finite upper limit to the population which can live upon a finite area (as of the earth) is really as much a physical as a biological matter. The amount of water which can be contained in a pint measure is strictly limited to a pint. It cannot by any chance be ten gallons.

3. *The lower absolute limit to population is zero.* Negative populations are in any common, practical sense, unthinkable.

4. History tells us what common-sense indicates *a priori*, namely that *each advancement in cultural level has brought with it the possibility of additional population growth within any defined area.* In the hunting stage of human culture the number of persons who can be supported upon a given area is small. In the pastoral stage of culture more persons can subsist upon a given area, though the absolute number is still small. In the general agricultural stage of civilization the possibilities of population per unit of area become again enhanced. The commercial and industrial stages of culture permit great increases of population, provided, of course, (and only under this condition) that there still remain somewhere else less densely populated areas where the means of subsistence can be produced in excess of local needs. In other words, each geographical unit which has been inhabited for any long time has, so far as the evidence available indicates, had a succession of waves or eras of population growth, each superimposed upon the last, and each marking the duration of a more or less definite cultural epoch.

5. *Within each cultural epoch or cycle of population growth the rate of growth of population has not been constant in time.* Instead the following course of events has apparently occurred generally, and indeed almost universally. At first the population grows slowly, but the rate constantly increases to a certain point where it, the rate of growth, reaches a maximum. This point may presumably be taken to present the optimum relation between numbers of people and the subsistence resources of the defined area. This point of maximum rate of growth is the point of inflection of the population growth curve. After that point is passed the rate of

growth becomes progressively slower, till finally the curve stretches along nearly horizontal, in close approach to the upper asymptote which belongs to the particular cultural epoch and area involved.

The curve may take a variety of forms, according to the number and forms of the terms which are retained in the exponent of e in the denominator of (i). Thus in the simplest form

$$y = \frac{k}{1 + me^{-{}'_{1}}} \tag{ii}$$

the curve describes the growth in a single epoch or cycle.

By retaining further terms the curve may be made to describe growth in more than one cycle, or cases where the population after rising through one or more cycles then declines. In one form the curve is asymmetrical on the two sides of the point of inflection, and is thus capable of describing skew growth. All of these characteristics of the curve have been fully discussed in the sources already referred to, and need not be elaborated here.

In order to give some idea of the fidelity with which this curve describes population growth a few examples may be presented. In these diagrams observed census counts of population are shown as small circles. The smooth line through these points is the fitted curve. This is drawn as a heavy, solid line through that portion of the total abscissal range within which there are observed points. Outside this range the curve is dotted, there being the extrapolated portions. Many other examples of the fitting of this curve, in addition to those presented here, are given in Chapter XXV. of "Studies in Human Biology" already referred to.

As a first example we may take the population growth of the United States. This is shown graphically in Fig. 1. The equation of the fitted curve is

$$y = \frac{197.27}{1 + 67.32e^{-.03134x}} \cdot \tag{iii}$$

It will be noted that in this case the observations all fall in the first half of the cycle. The agreement between observation and theory is obviously well-nigh perfect.

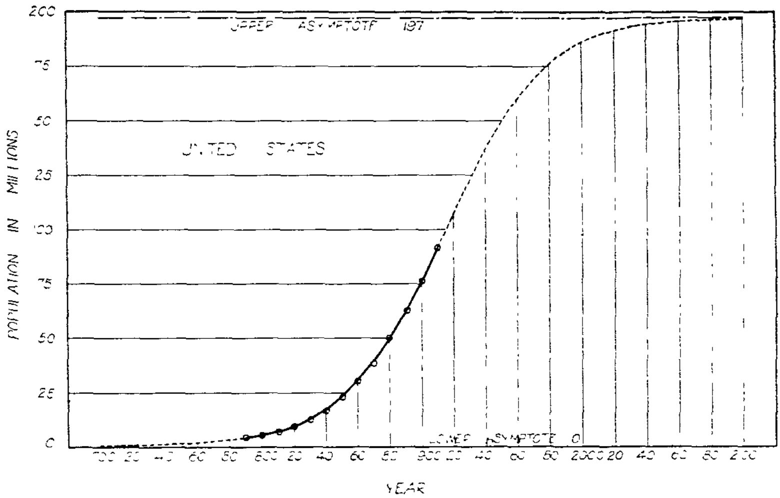

FIG. 1. The population growth of the United States.

The probable errors of the constants of the curve for the United States are as follows:

$$\begin{aligned} k &= 197.27 \quad \pm .55 \quad \text{millions of population,} \\ a_1 &= .03134 \pm .00013, \\ m &= 67.32 \quad \pm .17. \end{aligned}$$

As a second example we may take the population growth of France. This is of special interest, because the observed points all lie in the second half of the cycle, and indeed well towards the upper asymptote. The case is thus in the strongest contrast to that of the United States.

The equation to the fitted curve for France is

$$y - 6.604 = \frac{35.975}{1 + .8081 e^{-.01975x}} . \qquad \text{(iv)}$$

Again it is clear that the agreement between observation and theory is extremely close.

The probable errors of the constants of the curve for France are as follows:

$$\begin{aligned} k &= 35.975 \quad \pm .084 \quad \text{millions of population,} \\ a_1 &= .01975 \pm .00015, \\ m &= .8081 \quad \pm .0038. \end{aligned}$$

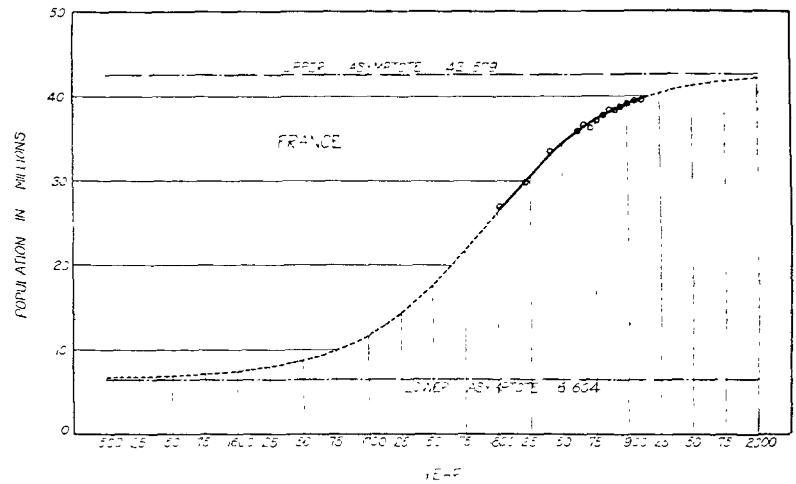

FIG. 2. The population growth of France.

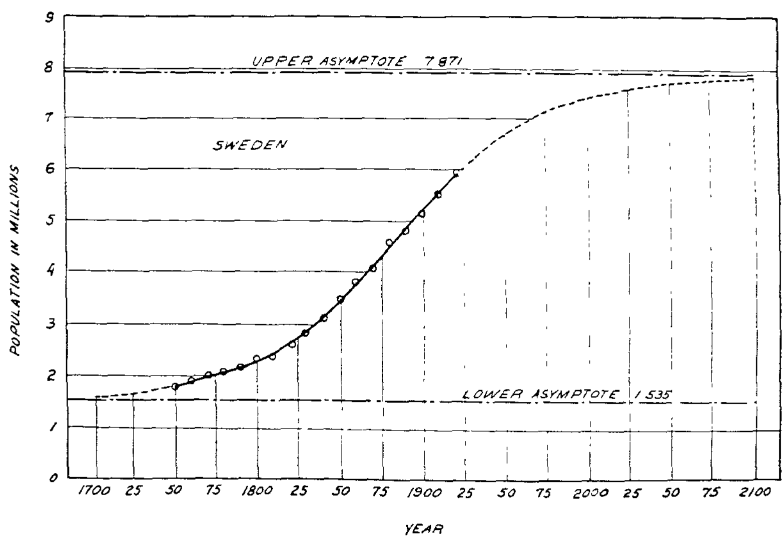

FIG. 3. The population growth of Sweden.

As a third example we may take the case of Sweden. This is of interest because it has the longest continuous set of census counts of population of any country. The curve of growth is shown in Fig. 3.

The equation of the fitted curve for Sweden is

$$y - 1.535 = \frac{6.336}{1 + 7.265e^{-.023006x}}. \qquad \text{(v)}$$

Here, with the long range of observations covering roughly two thirds of the entire cycle, the agreement between theory and observation is again extremely close.

The probable errors of the constants for the Swedish curve are as follows:

$$\begin{aligned} k &= 6.336 \quad \pm .054 \quad \text{millions of population,} \\ a_1 &= .023006 \pm .000019, \\ m &= 7.265 \quad \pm .016. \end{aligned}$$

So far all the examples have dealt with cases where all the recorded points fell within a single cycle of growth. Let us consider a case in which the known population history overlaps two cultural epochs. The best case of this sort available is Germany. Prior to about 1850 Germany had been chiefly in the agricultural stage of culture. The industrial stage had only begun in a small way. After 1870 there was a rapid and extensive development of industry, which reached such a stage by the time of the outbreak of the war in 1914 as to make Germany one of the most highly industrialized populations in the world. Can the curve of population growth here under discussion deal adequately with such a case?

There are two ways open to approach the problem. We may fit a single cycle curve like (ii) to the known population data before 1855, and another similar curve to the known data since 1855 and weld the two together at the point where they have a common tangent. The result of following this procedure is shown in Fig. 4, the equations to the two curves being:

For the period up to 1855,

$$y = 10.109 + \frac{34.036}{1 + 2.495e^{-.0394x}}. \qquad \text{(vi)}$$

From 1855 on,

$$y = 33.587 + \frac{82.944}{1 + 297.546e^{-.0472x}}. \qquad \text{(vii)}$$

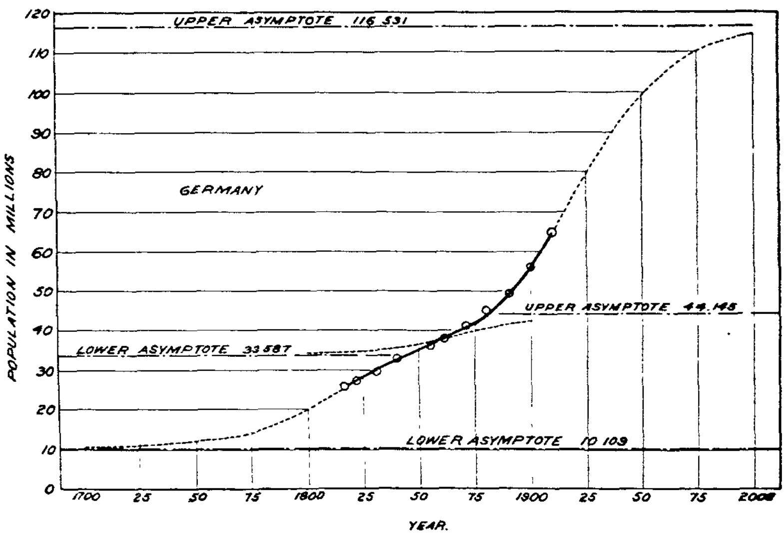

FIG. 4. The population growth of Germany fitted with two single cycle curves.

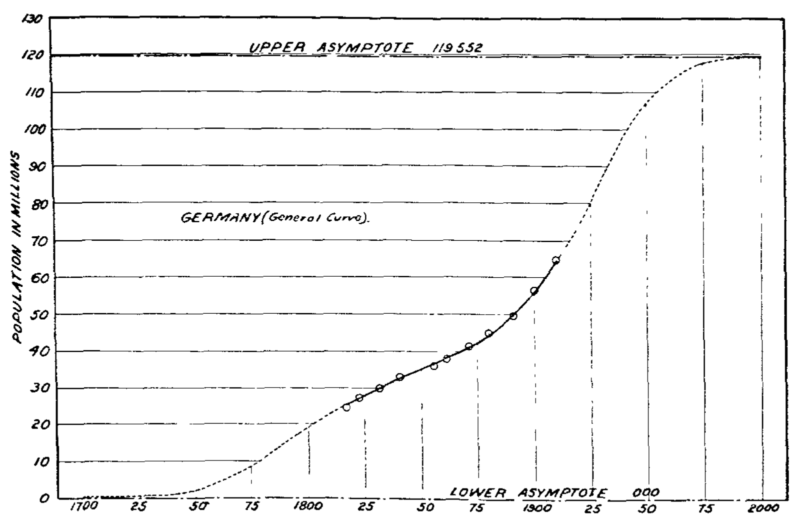

FIG. 5. The population growth of Germany, fitted with the general curve (equation (i)).

It is obvious that this procedure describes the known facts regarding Germany's population growth in an entirely satisfactory manner.

Can we do as well or better with the general curve (i)? The answer to this question is given by Fig. 5.

The equation of the fitted curve is

$$y = \frac{119.552}{1 + e^{1.9416 - .0331x + .00038x^2 - .0000021x^3}}. \qquad \text{(viii)}$$

Plainly the fit of theory to observations is as close as could be desired.

Taking all the available evidence into account, of which only a fraction has been presented above, I believe that it is justifiable to regard equation (i) as constituting a descriptive *law of population growth*. By its instrumentality we can describe how populations grow, and predict their future growth over reasonable periods of time with a degree of accuracy not attainable by any other method hitherto discovered.

THE NATION'S TRANSPORTATION PROBLEM.

By EMORY R. JOHNSON,

(Professor of Transportation and Commerce and Dean of the Wharton School, University of Pennsylvania.

It is a somewhat paradoxical fact that as the transportation facilities of any country are developed and made more efficient economic prosperity and social well being become increasingly dependent upon adequate, prompt and economical carriage of persons and property and transmission of ideas. Transportation is one of the principal determinants of social organization and social progress.

In a country such as the United States, the demand for transportation services grows with surprising rapidity, and the capital required to keep transportation facilities abreast of demand has already reached large figures and must necessarily continue to increase at a corresponding rate. Every nation has its transportation problem and always will have. The United States, having a more extensive transportation system than any other country possesses, may be said to have the greatest transportation problem of any nation.

The truth of this is evidenced by the facts as to the present and prospective traffic upon steam and electric railways and upon highways and the capital required for the development of railroad and other transportation facilities. The purpose of this paper is to state those facts briefly and to consider with equal brevity some of the principles that should control the government's relation to transportation.

Present Volume of Traffic.

Figures of traffic that run into billions are too large to give the human mind a definite impression, but they at least indicate in a general way the magnitude of the service rendered by railroads and other transportation agencies in the United States. In 1923, the railroads in this country had a revenue-paying traffic amounting to 413,562,000,000 tons one mile. If the non-revenue or company

freight be added the total becomes 457,589,000,000 ton miles. This is the largest freight traffic ever handled by the railroads in any year.

The passenger services of the railroads in 1923 amounted to 38,000,000,000 passengers one mile. This, however, was not so large a traffic as the railroads handled during the unusual period of the participation of the United States in the World War.

The electric railways of the United States in 1923 carried more than 10,000,000,000 passengers. Records are not kept of the distances that passengers travelled on electric railways and, for that reason, it is not possible to give figures for passenger miles.

It is not definitely known how many passengers were carried by motor busses in this country, but it is estimated that approximately 1,000,000,000 passengers ride in motor busses annually. The traffic is large and is increasingly rapidly. The Fifth Avenue Coach Company of New York City carried 56,000,000 revenue passengers in 1923. It is said that 3,500,000 people daily ride upon the busses operated on the streets of London. The motor busses in London are now transporting a billion and a quarter passengers annually. It has been roughly estimated that in 1923 passenger automobiles carried nearly ten billion passengers, and that the average ride was about eleven miles in length. This would make the motor passenger miles over one hundred billion or two and one-half times the railroad passenger miles.

Only very partial information is known of the volume of freight handled by motor trucks which are now being largely used both by organized carriers and by large shippers using their own trucks. An estimate based upon data presented by the Automobile Chamber of Commerce indicates that motor trucks in 1923 carried 2,220,000,000 tons of freight, the average haul being less than five miles. The total ton-miles was approximately ten and one-half billion.

Future Increase in Traffic.

A careful estimate has been made of the volume of traffic that will be moving upon American railroads ten years hence or in 1933. The estimate was made for the Bureau of Railway Economics by the railroad companies of the United States. The companies re-

porting information operated two thirds of the railway mileage in the country and handled over two thirds of the traffic. On the basis of these reports, it is evident that there will be an increase of at least one third in the freight traffic of American railroads during the decade ending in 1933. This is a much smaller rate of increase than has prevailed in the past. But even that growth will bring the revenge freight traffic of American railroads in 1933 up to 550,000,000,000 ton miles. The passenger traffic will probably increase somewhat slower than the freight business, but the increase during the decade ending in 1933 can hardly be less than 25 per cent. Even that conservative estimate brings the passenger traffic of American railroads in 1933 up to 47,500,000,000 passenger miles, which is not much more than the total reached during the war period.

It takes imagination as well as information to picture the economic development of a country such as the United States. The increase of large scale production accelerates industrial activity. Capital accumulates with augmented speed, the country goes ahead faster than most people realize. An indication of the economic growth of the country is given by a statement made by the Pennsylvania Railroad that upon the lines of that company's system 513 new industries were located in 1923 developing freight traffic estimated at more than 6,500,000 tons annually. These newly established enterprises on the Pennsylvania System will expect to load 186,000 cars annually.

In spite of the enormous travel by private automobiles and the rapid increase in the use of motor busses, the electric railways in the United States have a constantly expanding traffic. The increase in their traffic in the decade ending in 1922 was 60 per cent.

Although it seems somewhat paradoxical, it is none the less a fact that the increase of the use of motor busses tends to supplement rather than to supplant the service of electric railways. The busses bring traffic to the railroads and electric railways and thus complement, as well as compete with, the carriers by rail. Undoubtedly the most rapid increase in traffic is that of the motor busses, but figures are not available to show the actual rate of increase in this traffic.

Capital Required Annually.

This ever enlarging work of transportation cannot be performed without large expenditures of capital. The railroad companies in

estimating the growth of traffic for the decade ending in 1933 also estimated the minimum amount of capital that would be required to handle the prospective traffic. The estimate was $7,870,000,000 for a decade, or a capital investment in railroad facilities averaging $787,000,000 a year. This estimate is considered by many men to be much too small. Inasmuch as the investment in railroads at the present time probably exceeds $21,000,000,000, the above estimate of the annual requirements for the ensuing ten years is less than 3½ per cent. of the present invested capital. Certainly at least that percentage of new capital will be required.

How many hundred millions of dollars will be needed each year to provide electric railway facilities and to meet the demand for freight and passenger transportation by motor trucks and busses can only be conjectured. Apparently the development of transportation facilities in the United States during the next ten years will require not less than two billion dollars of capital annually. That amount will be in addition to whatever may be needed for the construction and maintenance of highways, for which $500,000,000 a year would be quite inadequate. These figures indicate that the people of the United States must expect to spend between two and one half and three billion dollars annually to provide transportation facilities.

Reasons for Private Ownership and Operation of Railroads in the United States.

The size of the transportation industries and the vital relation which transportation bears to economic prosperity and social well-being combine to give importance to the public policy adopted towards transportation. The first big question that the public must decide is whether transportation facilities should be provided by the government or by private enterprise. Fortunately this question seems for the present to have been decided in favor of private ownership and operation, but there is more or less widespread and continuous advocacy of government ownership and operation of railroads and other transportation agencies, in spite of the fact that sound economic and political philosophy as well as practical experience indicate that private ownership and operation of railroads and other transportation agencies is the wiser policy for the United States.

The reasons why private ownership and operation of the railroads are preferable to government enterprise are not always clearly understood. Briefly stated the main reasons are

(1) That private ownership and operation of railroads is in harmony with the genius of the people of this country. The country has gone ahead with amazing rapidity because the principle of private initative in economic life has been adhered to.

(2) The management of a railroad is an executive task for which a democratic government is ill adapted.

(3) The successful management of a railroad or other large business enterprise requires unity of thought and continuity of purpose on the part of the directors and executive officers. Government management is subject to legislature control which in a democratic country renders impossible that continuity of purpose essential to success in railroad management.

(4) Private enterprise invests funds in response to business needs. Private railroads are developed where there is the largest traffic demand. Government appropriations for railroads would be subject to political as well as economic influences.

(5) The officers and directors of a railroad company are under constant pressure to keep expenses down. They will employ no more than are needed. In government enterprises this is not the prevailing practice.

(6) Men of greatest efficiency and highest technical training are less attracted to government positions than to private business activities. If the railroads were owned and operated by the government, they would in a few years be operated by men of less efficiency than the men engaged in private enterprises.

(7) The expense of operating railroads under government management would be greater than under private management. Rates and fares would have to be higher or the tax payers would have to meet annually recurring deficits. With very few exceptions, government operation of railroads has resulted in deficits borne by the tax payers.

(8) The acquisition of the railroads by the United States government would double the present debt, large as that has been made by the recent world war. If the debt were doubled, the interest

rate would be increased. The financial burden borne by the taxpayers would be much heavier.

Government Policy—Essential Principles of Regulation.

Government regulation rather than government ownership and operation of the railroads and other transportation facilities is advisable in the United States. Government regulation is necessary in the public interest. It is also necessary that regulation should be in accordance with sound principles.

The public generally now has a much clearer understanding of the problem of government regulation of railroads than it had prior to the World War. Up to that time, the main purpose of government regulation of the railroads was to punish abuses in the railroad service and to prevent their recurrence. The Transportation Act of 1920, however, is framed in accordance with the principle that the government's relation to the railroads must be constructive and helpful as well as corrective. Most of the railroad abuses of the past have been eliminated. The primary aim of the government now is to assure adequate facilities.

The government cannot avoid regulating railroad rates and in the long run the federal and local government will inevitably regulate the rates of common carriers other than the railroads. This means that the revenues of railroads and other carriers will be determined by public policy. It needs no argument to prove that government regulation must not go so far as to prevent the carriers from making a fair profit upon their investment. New capital, and the amount required is large, must be secured from investors; and investors will place their capital only where there is hope of at least a reasonable return.

This fact is recognized by the Transportation Act of 1920 which directs the Interstate Commerce Commission to adjust and establish such railroad rates as will yield the carriers a fair return upon the property devoted to the service of the public. This is the heart of Section 15A of the Transportation Act, concerning which there has been much public misunderstanding. In the public interest, it is necessary that the principle of regulation underlying Section 15A should be maintained and developed.

Another general principle of regulation that has been partially recognized by legislation is that carriers serving the public should

work with each other instead of against each other. Up to the time of the World War, legislation sought to minimize the coordination of the railroads. Now their coordination is favored. The law contemplates the voluntary consolidation of the 200 large railroad systems and the 1,400 minor ones into a limited number of systems (possibly twenty). This is to be done without abandoning competition in services. The maintenance of competition in service is desirable and possible, and will not be prevented by the consolidation of railroads in accordance with the plan contained in the Transportation Act of 1920.

The desirable coördination of carriers includes not only the consolidation of our present railroads into a limited number of permanent systems of relatively equal strength, but also the correlation of railroads, waterways, and highways in such a way as to form a unified transportation system. The coördination of rail, water, and motor carriers involves the physical connection of railways and waterways and the interchange of traffic of the railroads and waterways with each other and with common carriers by motors. In the future, the aeroplane and the airways will have to be brought into the picture. What the country needs is a coördinated or unified transportation system, including all transportation agencies so regulated by the government as to permit carriers to serve the public with maximum efficiency and to provide the country, year by year, with the additional facilities required for its untrammeled economic development and for its progressively greater social well being.

PHYSICAL FACTORS IN PREDICTING THE BASAL METABOLISM OF GIRLS.*

By FRANCIS G. BENEDICT.

(*Read April 26, 1924*)

That there are reasonably close relationships between the size of the individual and the total twenty-four hour basal heat production and particularly between the surface area and the metabolism have for many years been considered by most physiologists as established facts. The original concepts were based upon startlingly few experimental data, however, and it was not until the collection of a large mass of metabolism measurements upon different normal individuals became available that closer study of these relationships could be made. After many urgent requests on the part of workers in metabolism, the Nutrition Laboratory in 1914 listed its measurements on normal individuals.[1] A consideration of certain of the factors affecting metabolism followed this presentation,[2] but the final analysis of the figures was designedly left until they could receive proper biometric treatment.[3] These original measurements have played a most important part in several discussions of basal metabolism. Du Bois and his collaborators have discussed the question chiefly from the standpoint of surface area,[4] finally recognizing the age and sex elements.[5] Dreyer [6] attributes special importance to weight and age, differentiating between males and females, while Gruber [7] has laid special emphasis upon length. Finally, the bi-

* From the Nutrition Laboratory of the Carnegie Institution of Washington, Boston, Massachusetts.

[1] Benedict, Emmes, Roth, and Smith, *Journ. Biol. Chem.*, 1914, *18*, p. 139.

[2] Benedict, *Journ. Biol. Chem.*, 1915, *20*, p. 263.

[3] Harris and Benedict, Carnegie Inst. Wash. Pub. No. 279, 1919.

[4] Gephart and Du Bois, *Arch. Intern. Med.*, 1915, *15* p. 835; ibid., 1916, *17*, p. 902; also Du Bois, *Am. Journ. Med. Sci.*, 1916, *151*, p. 781; also Du Bois, *Cornell Univ. Med. Bull.*, 1917, *6*, No. 3, Pt. 2.

[5] Aub and Du Bois, *Arch. Intern. Med.*, 1917, *19*, p. 831; also *Cornell Univ. Med. Bull.*, 1918, *7*, No. 3, 19th paper, p. 9.

[6] Dreyer, *Lancet*, 1920, Part 2, p. 290.

[7] Gruber, *Sitzungsberichte d. Bayerischen Akademie d. Wissenschaften*, 1921, p. 341.

ometric analysis made by the Nutrition Laboratory indicated that weight, stature, age, and sex all have independent relationships.[8] There has been a great deal of controversy and perhaps much time lost over comparisons of these several methods of predicting the metabolism from known factors. Since, however, practically all these analyses are based, in large part at least, upon the same experimental material, *i.e.*, the Nutrition Laboratory series, it is not surprising that the several prediction methods agree reasonably well among themselves.

The data originally published in 1914 included practically no children. But since that time two Nutrition Laboratory series of metabolism measurements on children, the one made in collaboration with Dr. F. B. Talbot[9] and the other with the coöperation of groups of Girl Scouts,[10] have made possible a more elaborate analysis of basal metabolism during the earlier years of human life. The prime use of basal metabolism predictions is obviously to compare the measured metabolism of pathological cases with the expected metabolism of a normal individual of a similar sex, age, and physical configuration. Perhaps no one problem is presented more frequently to the Nutrition Laboratory than the question as to what is the best method of predicting the metabolism of children, for in spite of the numerous metabolism measurements on normal children published from this Laboratory, they still fall far short of the number necessary to provide an ideal method for prediction applicable to that period of intense physical change represented between birth and 20 years of age.

Many children will be somewhat disturbed, if not actually agitated, during the first metabolism measurements, and it is obvious that only repeated tests can indicate the true basal metabolism. If, in any series of measurements, a period of complete repose gives us a value for the oxygen consumption or carbon-dioxide production that is lower than all the other period values, then if technical errors have

[8] Harris and Benedict, Carnegie Inst. Wash. Pub. No 279, 1919.

[9] Benedict and Talbot, Carnegie Inst. Wash. Pub. No. 302, 1921.

[10] Benedict and Hendry, *Boston Med. and Surg Journ.*, 1921, *184*, pp. 217, 257, 282, 297, and 329; and also, Benedict, Hendry, and Baker, *Proc. Nat Acad Sci.*, 1921, *7*, p. 10; Benedict, *Boston Med and Surg. Journ.*, 1923, *188*, p 127.

been avoided, this period must most closely approximate the basal metabolism.

With two or more closely agreeing low values the probable accuracy of the selected figure [11] must be very high, and the assumption is justified that the average of the two represents the closest approximation to basal and that *all other figures in all other periods are too high to be used in any basal discussion*, whatever their value may be as an index of the general, daily 24-hour metabolism which includes, naturally, periods of muscular and digestive activity. For comparing different children and for comparing children with youths and adults, basal figures alone can be employed.

Granted that muscular and digestive activity can be, if not wholly eliminated, at least in large part suppressed, a close analysis of the factors affecting metabolism shows that we have to deal with several known and probably many as yet unknown factors. With adults certainly biometric analysis has shown that weight, height, age, and sex each have an independent influence upon metabolism. The question then arises,—what factor or combination of factors is most closely correlated with the measured basal metabolism? Recent researches indicate that undernutrition plays an important rôle, certainly with adults and probably with children. Consequently it is highly probable that a simple statement of the age, height, weight, and sex will only partly meet the requirements of a prediction method and that one should give careful attention to the state of nutrition. Innumerable anthropometric records of children have been made and various indices as to the state of nutrition have been reported.[12] Their possible usefulness in predicting metabolism should be investigated.

In the prediction of the metabolism of children, one usually is confronted with the difficulty of expressing in an intelligent manner

[11] The pulse rate, if carefully taken, and particularly the graphic records of the degree of repose are invaluable as supporting evidence in the selection of minimum values

[12] Van der Loo, *Nederl. Tijdschr. v. Geneesk.*, Amst., 1910, *1*, p. 447 (cited in *Journ. Am. Med. Assoc.*, 1919, *72*, p. 1403); Rohrer, *Munch. med. Wochenschr.*, 1921, *68*, pp. 580 and 850; Pirquet, *System der Ernahrung*, Berlin, Vols. 1–4, 1917–1920 (also, *Zeitschr. f. Kinderheilk.*, 1916–1918, Vols. 14–18); Pirquet, "An Outline of the Pirquet System of Nutrition," Philadelphia and London, 1922.

the state of nutrition, either in terms such as "moderately well nourished," "well nourished," "thin," or "emaciated" (these having really no quantitative significance), or by the use of one of the many suggested indices of nutrition which involve measurements that are frequently not taken or not reported or that cannot be easily estimated from commonly reported measurements. Of the various indices of nutrition proposed by different writers, none has been so extensively compared with actual nutrition measurements as the index recommended by Pirquet. A recent visit to Pirquet's clinic in Vienna brought to mind again the possibility of correlating some of these indices with the basal metabolism, with the primary object of seeing if it was possible to secure a simpler method of prediction. No critics of Pirquet's system [13] can fail to admire the ingenuity with which he has devised his plan, particularly when one considers that it was primarily a war measure, that an untried group of laboratory and hospital assistants had to be used, only limited supplies of food were available, and the strictest economy in rationing was essential. That it served its purpose is attested by the large number of lives salvaged by Pirquet in his masterly experiment with the Viennese young during the period of stress incidental to the war and its aftermath. The true worth of much of his numerous, more or less arbitrary assumptions and dicta, including a terminology that v. Leersum [14] in a spirit of raillery calls "argot," may be safely left to the final assessment of his pediatric colleagues, but a close analysis of the factors entering into Pirquet's most ingenious, if not indeed (for America at least) practical, system shows two important conceptions that no physiologist can ruthlessly cast aside and condemn because he is not in favor of adopting the "nem system" in its entirety.

Very strongly impressed with certain relationships which he finds between the sitting height and the body weight of humans of all ages, Pirquet suggests a mathematical expression of the relationship between the weight and the sitting height determined by dividing the cube root of ten times the weight in grams by the sitting height

[13] Pirquet, *System der Ernahrung*, Berlin, Vols. 1–4, 1917–1920 (also *Zeitschr. f. Kinderheilk.*, 1916–1918, Vols. 14–18).

[14] v. Leersum, *Bull. Soc. Sci. d'Hyg. Alimen.*, 1921, *9*, p. 480.

in centimeters.[15] This quotient, to which Pirquet has given the characteristic name "pelidisi," is found to range from about 80 to 113 or 114, emaciated individuals showing the lower values and fat individuals the higher.[16] Consequently the pelidisi itself, Pirquet maintains, is a much better expression of the state of nutrition than is the old, admittedly wholly inadequate expression that the person is well, moderately, or poorly nourished. As a further assistance in indicating the state of nutrition of individuals, particularly children, he proposes an expression, likewise in the form of a coined word, which will indicate immediately the condition of the blood supply to the skin, the amount of fat, the turgor, and the muscular development.

In place of either of the two main factors heretofore considered by various writers as controlling the metabolism, *i.e.*, the mass of active protoplasmic tissue and the surface area of the body, Pirquet now proposes the surface of the intestine (Ernahrungsoberfläche). This conception challenges attention if for no other reason than that it is a welcome diversion from the time-honored notion that the metabolism is "determined" by the heat lost from the surface of the body. The measurement of the length of the intestine is out of the question, but from several physiological and geometrical standpoints and reasonings he comes to the conclusion that the absorbing intestinal surface bears a simple, geometrical relation to the square of the easily measured "sitting height." Hence this anthropometric measurement becomes one of the most important factors in his whole system. Without entering into the details of the method of computation and the various assumptions involved in the establishment of a seeming proportionality between food needs and sitting height (assumptions quite out of harmony with much of his reasoning),

[15] In this connection the importance of actual measurements of height without shoes and weight without clothing in all data for use in metabolism measurements should be strongly emphasized. Normal material, to be used as a criterion for hospital comparisons, must have a uniform basis which will not be affected either by variability in the weight of clothing (caused by custom, affluence or season) or by variations in the height of heels. For metabolism records only nude weights and heights without shoes are of value.

[16] It is important to realize that 100 does not signify perfect or normal in all ages.

the suggestion still remains—Is the sitting height a measurement that is intimately related in any way to the basal metabolism?

A series of basal metabolism experiments, including records of heat production, age, sex, height, sitting height, and weight, large enough to permit statistical treatment is most desired. The extensive Nutrition Laboratory series on individual children is deficient in that the sitting height was not taken.[17] Professor Pirquet, on a visit to the Nutrition Laboratory, stated, however, that the sitting height may be accurately obtained by adding 5 centimeters to one half of the total height. A number of measurements made in the Nutrition Laboratory have in general confirmed this, and consequently it has been possible for the purposes of this study to derive the probable sitting height of the children from the recorded total height.

Prior to the suggestion of a relationship between sitting height and metabolism, extensive comparisons of the influence of weight and surface area in the prediction of metabolism had been made by Benedict and Talbot[18] from the measured metabolism of the Nutrition Laboratory series of boys and girls. As the method of compu-

[17] Dr. C. B. Davenport has suggested to the writer that the sitting height, not to the vertex but to the suprasternal notch, might be found to be of importance in metabolism studies, as giving a measurement of trunk length uncomplicated by head length This measurement is strongly recommended and should be recorded by all workers in normal metabolism studies. Although we have not found material differences in the measurements of the sitting height, when carried out without special precautions and when compared with the specifications of Dreyer, for the sake of uniformity and exactness in description the Dreyer method is undoubtedly preferable. Dreyer (see Dreyer and Hanson, "The Assessment of Physical Fitness," New York, 1921, pp 5 and 6) shows a special apparatus which, however, is hardly necessary. He states that the subject should sit on the floor and not hang the legs over a chair or bench. He specifies that the measurements should be made as follows. "The subject places the backs of the fingers upon the platform on which he sits, and, with the fingers pointing backwards and the knees flexed, lifts the lower portion of the body gently backwards until the lowest bony portion of the os sacrum is in contact with the front of the measuring standard The back is then straightened until the back of the head comes into contact with the standard. It will be found that different persons require to bend the knees in different degrees in order to achieve this position. The head should be tilted neither up nor down, and the eyes should look straight forward. The measurement thus obtained gives the distance between the ischial tuberosities and the top of the head."

[18] Benedict, *Boston Med. and Surg. Journ.*, 1919, *181*, p. 107; Benedict and Talbot, Carnegie Inst. Wash. Pub. No 302, 1921.

tation and comparison is, in lieu of statistical treatment, the most rational that we have thus far found, a word as to its details is here desirable. For example, basal metabolism measurements were made in a respiration chamber on a total of 128 boys and 114 girls, and simultaneously the heights, weights, and measured surface areas were recorded. The result for each child (boy or girl) was represented by a point on a chart, with the values for the measured metabolism as the ordinates and another factor, such as weight, age, or surface area, as the abscissae. Using separate pieces of tracing paper, five different members of the Nutrition Laboratory staff (all skilled workers with metabolism charts) each sketched, freehand, the curve that to him seemed to represent most truly the general trend of the plots. A composite of these five curves was subsequently made, to obtain the final curve represented on the chart. Thus a curve was obtained showing the predominant trend of the metabolism as related to its compared factor.

Precisely this treatment was accorded the relationships between the basal 24-hour heat production and body weight, measured surface area, and age.[19] These curves show the general trend of the metabolism as referred to weight, surface area, and age, and from the general grouping of points about the curves it would seem that certain curves might be of practical value for predicting the unknown basal metabolism of a boy or girl whose weight or surface area is known. Although the curves represent trends only, the wide scatter of individual points is disturbing to the thought of using such curves for prediction purposes. Furthermore, the percentage deviation of the points from the smoothed curve is largely affected by the actual weight or surface area of the individual. Percentage relationships only, therefore, may properly be used in assessing value to these curves for prediction purposes.

The actual heat production of a child, as measured, is referred, for example, to that noted on the curve for the weight of the particular child. The difference between these two values indicates whether the predicted heat production (from the smoothed curve) is greater or less than the actual heat production. Thus, if the predicted heat

[19] Benedict, *Boston Med. and Surg. Journ.*, 1919, *181*, p. 107; Benedict and Talbot, Carnegie Inst. Wash. Pub. No. 302, 1921.

production is lower than the actual, the difference will be expressed by a minus sign. The percentage difference is obtained by dividing 100 times the difference by the actual heat production. Thus, in a typical case, a boy 7 years and 2 months old, with a nude weight of 19.9 kilograms and a height of 111.5 centimeters, had an actual basal heat production per 24 hours of 838 calories. From the smoothed curve showing the relationship between the basal 24-hour heat production and the weight a boy weighing 19.9 kilograms would be expected to have a heat production of 857 calories, *i.e.*, the predicted heat production is 2.3 per cent. above the actually measured metabolism.[20] Similarly, on the basis of the curve for total calories referred to body surface, this same boy, having a surface area (measured by the Du Bois linear formula) of 0.82 square meter, is found to have a predicted heat production of 867 calories, which is 29 calories or 3.5 per cent. greater than his actual metabolism. The results for boys and girls were calculated in this manner and expressed on the percentage basis, the total heat production being referred both to weight and surface area.

The next step in the analysis is to note the average percentage deviation, without regard to sign, of all of these predicted values from the values actually found. Obviously that method of prediction giving the smallest deviation of predicted from actual is the best. The extensive tabulations and details of all calculations are given in the original report[21] of these studies, and a repetition here is

[20] The method of computation of the percentage values given in this paper may be challenged. Thus, we have used the actual heat production as the basis in determining the percentage deviations, that is, we have divided the difference between the actual and the predicted metabolism by the actual metabolism. One may maintain that we should have used not the actual but the predicted heat production as a basis, since the true test of the prediction curve is the range of the deviations from the prediction. This would mean that some of the percentage values previously computed on the basis of the actual metabolism would be raised a little and some would be lowered a little. There would therefore be compensation here and the *average* percentage deviation would be altered but slightly on this new basis. We have accordingly not deemed it necessary to recompute these percentages, although, strictly speaking, it would have been more correct to have used the predicted metabolism as the basis.

[21] Benedict and Talbot, Carnegie Inst. Wash. Pub. No. 302, 1921, pp. 187 et seq.

undesirable. A summary of the findings is, however, given in Table I.

TABLE I.

Comparison of the Actual Basal 24-Hour Heat Production of Boys and Girls with the Metabolism Predicted from Body Weight and Body Surface.

Group.	Basis of Prediction.	Number of Subjects.	Deviation of Predicted from Actual.
			± p.ct.
All boys........	Weight	128	7.4
Below 10 kilos........		60	8.7
Above 10 kilos........		68	6.3
All boys	Surface	128	7.5
Below 0.45 sq. m........		52	7.7
Above 0.45 sq. m........		76	7.3
All girls........	Weight	114	9.7
Below 10 kilos........		58	11.8
Above 10 kilos........		56	7.5
All girls........	Surface	114	9.8
Below 0.45 sq. m........		49	11.6
Above 0.45 sq. m........		65	8.5

In Table I. two facts are clearly brought out, first, that the predictions below 10 kilograms or 0.45 square meter are with both boys and girls poorer than those made above these limits. Secondly, on any basis the prediction for boys in general is measurably better than the prediction for girls upon the same basis. There is a distinctly better prediction of the basal metabolism of children over one year of age (*i.e.*, over 10 kilograms or 0.45 square meter) from the weight rather than the surface area. The best prediction is that for boys over 10 kilograms, *i.e.*, ± 6.3 per cent. From these summarized analyses of the deviations of predictions with boys and girls it was concluded that the prediction from weight was the best available, and a table was prepared, giving the most probable heat production of boys and girls of different weights from birth to puberty. This table has been published [22] and is now much used as a method of predicting the basal metabolism of children.

[22] Benedict and Talbot, Carnegie Inst. Wash. Pub. No. 302, 1921, Table 36, p. 206; see, also, Carpenter, Carnegie Inst. Wash. Pub. No. 303, 1921, Table 20, p. 109.

Pirquet's concept of a proportionality between metabolism and the sitting height, a concept borne out by his practical experiences in feeding large numbers of children in Vienna, seemed to warrant a closer examination of the relationship between the measured basal metabolism and sitting height with these children. From the Nutrition Laboratory data for children this relationship between metabolism and sitting height[23] could be easily computed. It was found, in considering the series of boys over 10 kilograms, that dividing the measured 24-hour basal metabolism by the square of the sitting height gave a factor corresponding to 0.241 on the average. The range between different individuals was very great. For example, the factor for one boy was as low as 0.165 and for three individual boys as low as 0.186, while with five it was 0.310 or above, the highest value being 0.340. Thus we see that this constant had an extreme variation of over 100 per cent. It became clear at the outset, therefore, that while a large number of boys showed constants between .200 and .240, the variation was very much wider with many others. Any concept of this "constant" representing a single value must therefore immediately be given up, for whatever semblance of proportionality Pirquet found with his Viennese children between their total food needs and sitting height, certainly no significant relationship could be established on the basis of basal metabolism.

At this step it became evident that the severest test of any proposed method of prediction would be made by applying the prediction method to our measured series of girls. The predictions for boys first made, either from the weight or even by the Harris and Benedict formula for men, have been shown to be reasonably satis-

[23] It is important to emphasize that in this paper wherever sitting height is mentioned, it does not refer to actually measured sitting height, but it represents one half of the total stature in centimeters plus 5 centimeters The propriety of this procedure suggested by Pirquet is fully established in numerous series of measurements by Bardeen (*Am. Journ. Phys. Anthropol.*, 1923, *6*, p. 355). Dr. Fritz B. Talbot has called my attention to the fact that this rule, which may work admirably for normal children, is notably wrong with a series of Mongolian idiots which he has measured, in which the additive factor is much nearer 10 centimeters rather than 5 centimeters. The applicability of this formula, therefore, must always be somewhat restricted. Direct measurements are of course by far the safer procedure.

factory.[24] With girls the prediction is by no means satisfactory and many of the problems in metabolism presented to the Nutrition Laboratory have had to deal with the metabolism of girls. Attention was therefore immediately directed toward the series of girls and the possible relationships between the heat production and sitting height were studied with them. When the values for the total heat production (as measured) divided by the sitting height squared were plotted, in one chart against the height, in another against the sitting height, and in another against the sitting height squared, and when through these plots curves representing the probable general trend were drawn in accordance with the method outlined previously, it was found that the prediction from these curves was of a much higher order of accuracy than the prediction by any other method thus far proposed for the girls. Indeed, as can be seen from Table I., the best predictions there recorded showed an average deviation of predicted from actual, on the basis of weight, of ± 7.5 per cent. for the 56 girls above 10 kilograms in weight, and on the basis of surface area ± 8.5 per cent. with the 65 girls above 0.45 square meter. We now find that by using the relationship between sitting height squared and the total heat and referring to the three bases, height, sitting height, and sitting height squared, it is possible to secure predictions for these 56 girls weighing 10 kilograms and above of an order of ± 6 per cent. It is quite clear, therefore, that the sitting height squared, *or at least the length factor*, has materially altered the entire complexion of the method of prediction.

As a result of this finding a large number of computations and comparisons were made, using chiefly the data for the 56 girls in the Benedict-Talbot series weighing 10 kilograms or above. It is impracticable to present all of the numerous tables and plots, but in Table II. are summarized the average percentage deviations found by predicting the metabolism from the several curves.

A general inspection of this table shows that the higher percentage deviations are noted almost invariably with relationships which involve body weight or area, such as those under the head of heat per kilogram or heat per unit of surface area. On the other

[24] Benedict and Talbot, Carnegie Inst. Wash. Pub. No. 302, 1921, p. 194; also Benedict, *Proc. Nat. Acad. Sci.*, 1920, *6*, p. 7.

TABLE II.

COMPARISON OF DIFFERENT METHODS OF PREDICTING THE BASAL 24-HOUR HEAT PRODUCTION OF THE 56 BENEDICT-TALBOT GIRLS 10 KILOGRAMS AND OVER IN WEIGHT.

(Values represent average percentage deviations, without regard to sign, of predicted from actual metabolism.)

Heat Factor.	Basis of Reference.				
	Age.	Weight.	Height.	Sitting Height.	Sitting Height (Squared).
Total heat [25]	6.2	7.5	6.0	6.7	5.8
Total heat divided by height	5.8	6.2	6.1	5.9	6.1
Total heat divided by sitting height	5.7	6.5	5.9	6.0	6.3
Total heat divided by sitting height (squared)	7.2	7.5	6.1	5.9	5.8
Total heat divided by weight divided by height	7.5	....	....	....	....
Heat per kilogram	9.4	7.4	[26]	[26]	[26]
Heat per kilogram divided by height	11.0	6.9	7.7	7.8	8.1
Heat per kilogram divided by sitting height	10.9	6.6	7.3	10.8	7.6
Heat per kilogram divided by sitting height (squared)	13.2	7.8	7.0	7.9	8.6
Heat per square meter	8.4	8.0	8.1	....	....
Heat per square meter divided by sitting height (squared)	....	....	....	8.0	....
$\frac{\text{Total heat}}{\text{Height}}$ divided by weight	[26]	....	....	....	....

[25] The prediction on the basis of the total heat referred to body surface showed an average percentage deviation of $\pm$ 7.9 per cent. The values for total heat production were also plotted with reference to the Van der Loo index (weight divided by square of length), but the scatter of the points was very wide.

[26] A chart was plotted on this basis of reference, but the scatter of the individual points was so great that it was not considered justifiable to draw a smoothed curve through the points and make the computations therefrom.

hand, in those relationships which involve some length factor, such as when the total heat is referred to either height, sitting height, or sitting height squared, and when the total heat is divided by the height, by the sitting height, or by the sitting height squared, and referred to the several other factors, the order of prediction is very much improved. Of the half hundred methods of prediction listed in Table II., a goodly number show an accuracy of $\pm$ 6.3 per cent. or better, and in limiting the selection of the best method of pre-

diction to those methods showing an average deviation of predicted from actual of ±6.3 per cent, or better, we see that all prediction methods involving the weight element and the surface area element are, except in one instance, immediately ruled out. But even with the limited selection, the determination of the most practical or best method of prediction among this selected group is still somewhat involved and we have recourse to another factor, namely, the distribution of the individual percentage deviations outside of the average deviation.

In Table III., the data are collected for those bases of prediction only with an average accuracy of ± 6.3 per cent. or better. This tabulation shows the actual number of girls in the series of 56 studied whose deviations in predicted metabolism are outside the average deviation, the number outside ± 10 per cent., and the number outside ± 15 per cent.

A general inspection of the table shows that the range of accuracy in prediction is from ± 5.7 to ± 6.3 per cent., with a central tendency for the average to be about ± 6.0 per cent. The number of individuals outside the average deviation is very close to 22 or 25, the number outside the ±10 per cent. range is from 9 to 15, and the number outside ± 15 per cent. from 3 to 5. Seemingly there is but little to differentiate the best method, owing to the small (statistically speaking) number of individuals that we are working with, and hence the advantage, from the practical standpoint, must lie with that method involving the simplest measurements. As the height is invariably recorded and it is necessary for this series of girls to compute the sitting height from the height, the relationship between the total heat production and the height is selected as the most practical method of predicting, although it does not give the absolute minimum value in the accuracy of prediction, as shown in Table III. The average error is ± 6.0 per cent., there are 23 girls outside the average deviation, 11 girls outside ± 10 per cent., and 4 girls with a prediction error of ± 15 per cent. or more.

The curve for the total heat production referred to height is given in figure 1, the data for 57 girls under 10 kilograms in weight being plotted as well as the data for the 56 girls above 10 kilograms in weight. Through the plotted points representing the total heat

as measured and the stature has been sketched the curve representing the general trend of the metabolism, as outlined by five workers independently. In figure 1 we have not indicated the girls below 10 kilograms by special points, but in general those girls below 78 centimeters in height are those weighing under 10 kilograms.

TABLE III.

DISTRIBUTION OF PERCENTAGE DEVIATIONS IN THOSE PREDICTIONS WHICH SHOWED AN AVERAGE ACCURACY OF ± 6.3 PER CENT. OR BETTER FOR THE 56 GIRLS 10 KILOGRAMS AND OVER IN WEIGHT.

Basis of Prediction.	Average Deviation of Predicted from Actual.	Number of Girls Outside of: Average Deviation.	± 10 p.ct.	± 15 p.ct.
	± p.ct.			
Total heat referred to age	6.2	23	10	5
Total heat referred to height	6.0	23	11	4
Total heat referred to sitting height (squared)	5.8	24	9	3
$\frac{\text{Total heat}}{\text{Height}}$ referred to age	5.8	23	12	3
$\frac{\text{Total heat}}{\text{Height}}$ referred to weight	6.2	25	14	4
$\frac{\text{Total heat}}{\text{Height}}$ referred to height	6.1	24	12	4
$\frac{\text{Total heat}}{\text{Height}}$ referred to sitting height	5.9	22	9	5
$\frac{\text{Total heat}}{\text{Height}}$ referred to sitting height (squared)	6.1	25	15	4
$\frac{\text{Total heat}}{\text{Sitting height}}$ referred to age	5.7	22	11	3
$\frac{\text{Total heat}}{\text{Sitting height}}$ referred to height	5.9	24	12	4
$\frac{\text{Total heat}}{\text{Sitting height}}$ referred to sitting height	6.0	24	12	4
$\frac{\text{Total heat}}{\text{Sitting height}}$ referred to sitting height (squared)	6.3	23	15	4
$\frac{\text{Total heat}}{\text{Sitting height squared}}$ referred to height	6.1	23	13	4
$\frac{\text{Total heat}}{\text{Sitting height squared}}$ referred to sitting height	5.9	25	14	3
$\frac{\text{Total heat}}{\text{Sitting height squared}}$ referred to sitting height (squared)	5.8	23	13	3

From this curve we have also predicted the metabolism of the 57 girls below 10 kilograms in weight, and find that the error of prediction is on the average ± 10.9 per cent, with 28 individuals falling outside the average and 16 outside of ± 15 per cent. This average accuracy of ± 10.9 per cent. betters appreciably the ± 11.8 per cent. recorded in Table I. for the 58 girls below 10 kilograms in weight,

when the prediction was based on weight,[27] and is also better than the ± 11.6 per cent. found when the prediction was based on surface area.

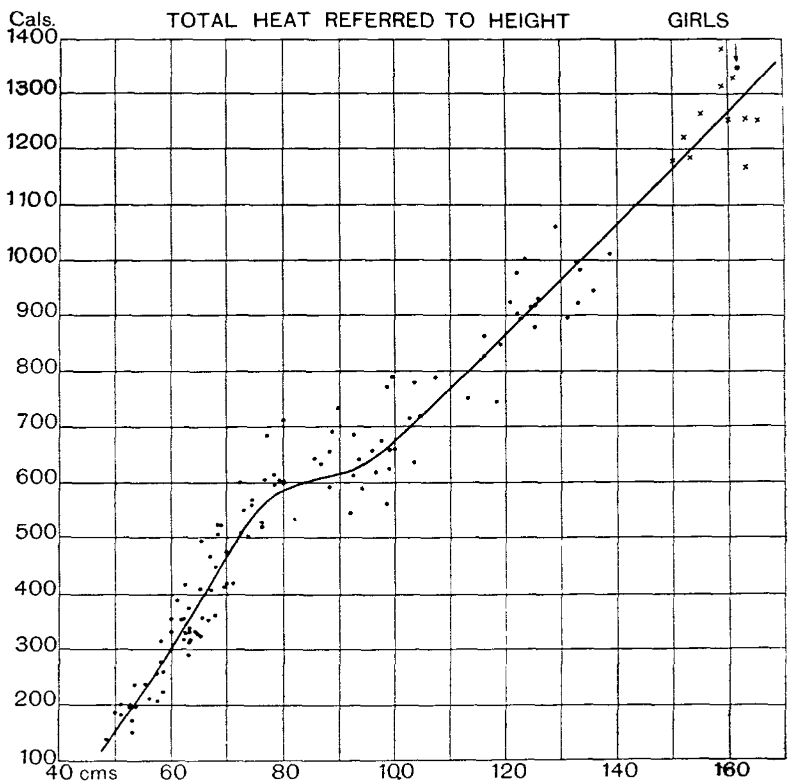

FIG. 1. Basal heat production of girls per 24 hours referred to height. The dots represent girls from 1 week old to 12 years old; the crosses represent the groups of Girl Scouts: the arrow indicates the average value for 103 adult women.

The curve in Fig. 1, therefore, by every test proves its superiority to the other methods of predicting the metabolism of girls from birth to 12 years of age, and it is clear that Benedict and Talbot, instead of stressing the weight or surface area, should have recognized the great significance of length, either in its simpler form as total stature or as

[27] On the weight basis, data were available for 58 girls below 10 kilograms, but on the height basis the data for only 57 girls were available, as no height was recorded for one of the girls.

sitting height, which was so much emphasized by Pirquet and later by Dreyer. With children from birth to puberty, to be sure, one would expect to find a close relationship between age and height and weight, but while age and weight are both related to heat production, the heat is somewhat more closely related to height than to either age or weight.

Between puberty and 21 years of age, when the adult formula for the prediction of the heat production of women applies,[28] there is still a period of 8 years to be considered, which is represented by the Nutrition Laboratory measurements of Girl Scouts.[29] These Girl Scout values have been plotted in Fig. 1, which indicates the general trend of the total heat production when referred to height, and the curve for girls up to 12 years of age (or 138 cms. in height) has been extended to pass through the cluster of Girl Scout data. The consideration as to whether this section of the curve may be used for predicting the metabolism between 12 and 21 years of age brings out two facts. In the first place, when one attempts to use this curve to predict the metabolism of the Girl Scout groups, one finds that the 11 *groups* are predicted on the average with an error of ± 4.2 per cent., with 3 groups outside the average and one group more than 10 per cent. from the central line. This accuracy of prediction is measurably poorer than the accuracy found (i.e., circa ± 3.4 per cent.) when, as in the original treatment of the Girl Scout data, weight, age, and surface area alone were considered and the prediction curve was based only on the Girl Scout data, with no attempt to blend the curve with the curve for the younger girls.

A second and more important fact is to be considered in determining whether this curve showing the heat-height relationship is the best one to use in predicting the basal metabolism of girls in this age range. From the Girl Scout measurements we find that all the groups of girls (each group containing 12 girls) between 12 and 18 years showed essentially the same total basal metabolism,

[28] Harris and Benedict studied a few girls under 21 years of age, but their prediction tables do not apply below 21 years.

[29] Benedict and Hendry, *Boston Med. and Surg. Journ.*, 1921, *184*, pp. 217, 257, 282, 297, and 329; also Benedict, Hendry, and Baker, *Proc. Nat Acad. Sci.*, 1921, *7*, p 10; also, Benedict, *Boston Med. and Surg. Journ.*, 1923, *188*, p. 127.

the average for all the groups being not far from 1,250 calories. Since the younger girls were likewise smaller in weight, shorter, and of smaller surface area, the heat production per kilogram of body weight and per square meter of body surface was specifically high with the younger, i.e., shorter and lighter weight girls. From Fig. 1, in which the total metabolism is referred to height, it is clear that as height increases total metabolism increases, while the Girl Scout findings show that the total metabolism remains essentially constant between 12 and 18 years of age in spite of an increase in stature from 150 to 165 centimeters. The curve for total metabolism referred to height implies a continually increasing metabolism with increasing height, contrary to the Girl Scout findings, and hence the usefulness of the curve for predicting beyond 12 years (or 138 cms.) is immediately challenged. It is important to bear in mind that the direction of this curve was determined solely by the measurements on children, but it should be pointed out that the average heat production (1,349 calories) at the average height (162 cms.) of the 103 adult women studied by Harris and Benedict would lie about 4 per cent. above the curve as at present drawn. This average point for the 103 women is indicated in Fig. 1 by the arrow.

The recent publication by Miss Bedale [30] of a series of measurements of basal metabolism of young girls confirmed the Nutrition Laboratory findings of uniformity of total metabolism with the Girl Scouts. Thus, her data for girls show that on the average the basal heat production per 24 hours is 1,402 calories at 12 years, 1,478 calories at 13 years, 1,456 calories at 15 years, 1,363 calories at 16 years, and 1,349 calories at 17 years. From these data one can draw the conclusion that her girls 12 to 17 years old also had essentially the same basal metabolism, *i.e.*, not far from 1,400 calories. That this value is measurably above the 1,250 calories found for girls of similar ages in the Nutrition Laboratory series is not of significance at this time.[31]

[30] Bedale, *Proc. Roy. Soc., London,* 1923, Ser. B, *94,* Table I., p. 375.

[31] It should be pointed out here, however, that the discussion of Miss Bedale's data later in this text is based not upon these *average* values but upon *minimum* figures selected from the detailed data kindly placed in our hands by Miss Bedale.

In an unpublished series of metabolism measurements on girls made by Miss Grace MacLeod of the Department of Nutrition, Teachers College, Columbia University, New York City, certain pertinent data are also to be found. The detailed data have generously been placed at the disposal of the Nutrition Laboratory and from these data it is found that the basal 24-hour heat production was on the average 1,295 calories at 12 years, 1,365 calories at 13 years, and 1,308 calories at 14 years. In other words, among her 12-, 13-, and 14-year-old girls there was also a uniformity in the basal metabolism, a fact which further confirms the findings from the Girl Scout data and Miss Bedale's data.

Thus three independent series of measurements are in agreement in the finding that the basal metabolism is essentially constant with girls from 12 to 18 years of age, *i.e.*, independent of weight, height, or age. To neglect this finding in a proposed method of prediction seemed wholly unjustifiable, and consequently still another method of prediction was studied in an attempt to throw light upon this problem.

In many of the numerous curves it was often noted that those girls farthest away from the central tendency were quite apt to be somewhat over or under weight, although in the original selection of material every precaution[32] was taken to avoid obviously pathological children. This seeming correlation between the deviation from the general trend and the weight, or rather the weight-height ratio, brought up vividly Pirquet's second important point, namely, the classification of children according to some nutritive index such as, for example, the pelidisi.[33] The pelidisi for each child was therefore computed[34] and the values for the total heat production were then divided by the pelidisi. On plotting these quotients against increasing height (as shown in Fig. 2), it was found that there was a distinct tendency for a straight line relationship with

[32] See discussion and demonstration of normality of children used in the study (Benedict and Talbot, Carnegie Inst. Wash. Pub. No. 302, 1921, pp. 32 et seq.).

[33] Pirquet, "An Outline of the Pirquet System of Nutrition," Philadelphia and London, 1922, pp. 89 et seq.

[34] Gruber's stature index was also computed for each child and found to be rather closely correlated with the pelidisi, so it was deemed unnecessary to make any prediction on this basis.

girls from about 3 months of age, *i.e.*, 60 centimeters in height, up to and including the Girl Scout groups. It then seemed desirable to see what this relationship would be with the 103 normal women studied by Harris and Benedict. Their pelidisi were consequently computed and the factors for the total metabolism divided by the pelidisi were plotted (not shown in detail in Fig. 2), also referred

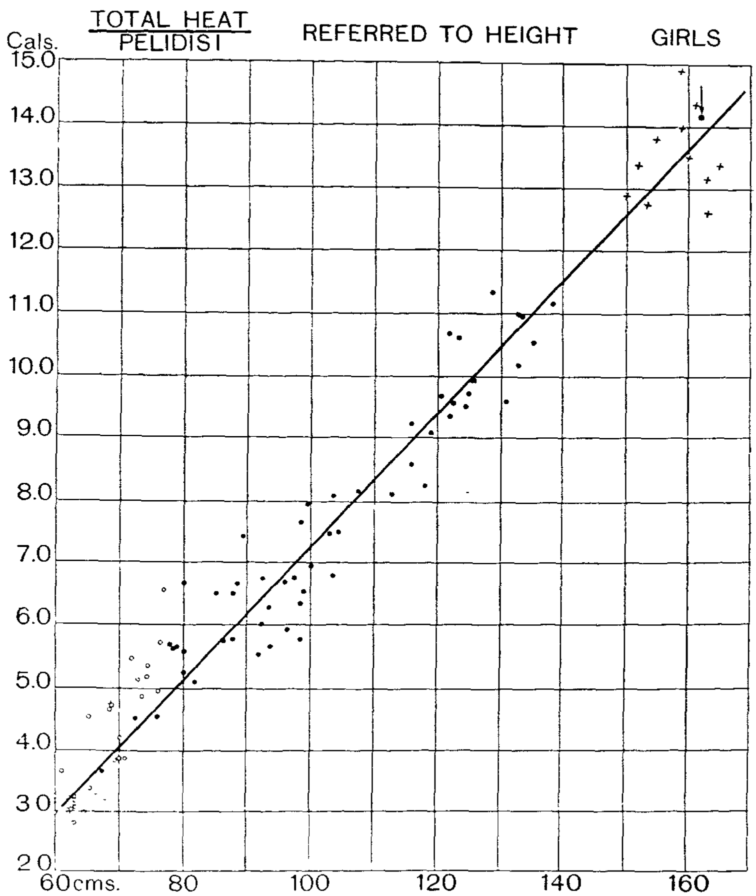

FIG. 2. Relationship between height and the basal heat production per 24 hours divided by the pelidisi, with females from 1 week of age to full maturity. The hollow dots represent girls below 10 kilograms in weight or younger than 1 year; the solid dots represent girls 10 kilograms or above in weight; the crosses represent the groups of Girl Scouts; and the arrow indicates the average value for 103 adult women.

to height. The scatter of the individual points was very wide, and yet the central tendency was clearly in line with the straight line found with the children, as shown by the average point for the women plotted in Fig. 2, which lies about 3 per cent. above the line. It is important again to emphasize that the line in Fig. 2 was drawn without any regard to the average value found for women.

In the entire age range, therefore, from 3 months through adult life a reasonably straight line relationship was found with girls and women for the total heat production divided by the pelidisi (a distinctly arbitrary procedure, to be sure) referred to height. The true significance of this finding appears later, when comparing the Nutrition Laboratory series of measurements on girls with other series. Using this straight line to predict the metabolism of the 56 girls in the Nutrition Laboratory series above 10 kilograms, we find an average error in prediction (see Table IV.) of ± 7.2 per cent.,

TABLE IV.

STUDY OF THE ACCURACY IN THE PREDICTION OF THE BASAL METABOLISM OF GIRLS FROM THE PELIDISI AND THE AGE, HEIGHT, OR SITTING HEIGHT (SQUARED).

Total heat[35] / Pelidisi referred to	Average Percentage Deviation (±).	Number of Girls Outside of		
		Average Deviation.	± 10 p.ct.	± 15 p.ct.
Age (56 girls 10 kg. and above)	6.3	22	11	4
Height (straight line function)				
56 girls 10 kg. and above	7.2	23	15	6
11 Girl Scout groups	4.8	4	1	0
103 women	7.1	45	28	11
Height (smoothed curve)				
57 girls below 10 kg.	10.6	26	27	16
56 girls 10 kg. and above	6.2	24	10	5
11 Girl Scout groups	4.9	5	1	0
103 women	7.2	43	28	13
Sitting height (squared)				
56 girls 10 kg. and above	6.5	23	13	3
11 Girl Scout groups	5.3	4	1	0
103 women	7.3	46	26	13

[35] The total heat divided by the pelidisi was also referred to weight, but the scatter of the individual points was too wide to justify the laying on of a smoothed curve and making predictions therefrom.

with 23 girls outside the average deviation, 15 outside ± 10 per cent., and 6 outside ± 15 per cent. This order of accuracy does not compare favorably with that for the prediction of total metabolism referred to height, but it is to be noted that *it is actually somewhat better than the heat-weight relationship previously suggested as the most practical for prediction purposes.*

Momentarily disregarding the straight line and by the usual procedure sketching a curve through the individual points representing the total metabolism divided by the pelidisi referred to height, we get a curve somewhat off the straight line, giving, however, an accuracy of prediction considerably better than on the straight line basis, *i.e.*, ± 6.2 per cent. for the 56 girls above 10 kilograms in weight.

For the ages represented by the Girl Scout data, *i.e.*, 12 to 18 years, the prediction from the pelidisi and the height has an order of accuracy for the Girl Scouts on the basis of the straight line relationship of ± 4.8 per cent., and on the basis of the relationship expressed by the smoothed curve of ± 4.9 per cent., values again larger than that obtained with the heat-height prediction curve of ± 4.2 per cent. and, correspondingly, larger than the value of about ± 3.4 per cent. obtained in the original Girl Scout predictions, on the basis of heat per kilogram of body weight and heat per square meter of body surface referred to age and weight.

For the 103 women the order of accuracy on the basis of the straight line relationship between height and total metabolism divided by the pelidisi is ± 7.1 per cent., *i.e.*, a somewhat wider deviation than the deviation of ± 6.3 per cent. obtaining in the prediction by the Harris-Benedict formula for adult women.[36]

While, therefore, from the standpoint of general physiology it is a significant fact that when the state of nutrition, as expressed by the pelidisi, is brought into play a straight line[37] represents the heat-

[36] Harris and Benedict, Carnegie Inst. Wash. Pub. No. 279, 1919, p. 227.

[37] It will be noted in Table IV. that we also predicted the metabolism from the pelidisi and age and sitting height (squared), but in our text discussion we have laid special emphasis upon the straight-line function with reference to height, as this implies a higher degree of physiological probability of correlation.

height relationship between 3 months of age and full maturity, for the practical purpose of predicting metabolism this relationship is not of so great value as the simple heat-height relationship for girls from birth to puberty, and for adult women of 21 years and above the prediction is better made by a formula considering age, height, and weight.[36]

There remains the age range from 12 to 20 years to be still further considered. The Girl Scout values have been marked as very low by many observers. In fact, the question has been raised as to whether the metabolism of girls in bed at 4 A.M., asleep, is not lower than that which is commonly called basal. This accentuates in a striking manner the criticism of Krogh[38] and suggests that his term "standard metabolism" is more logical. The conditions during the experiments with the Girl Scouts were admittedly different from those obtaining in basal metabolism measurements in the ordinary clinic or physiological laboratory. The girls were sound asleep, well covered, and thoroughly relaxed, in an environmental temperature of 20° C., and the lowest minimum value observed during the night for the group was selected as the basal value. On the other hand, the measurements were not made the traditional 12 hours after the last food. Yet the relatively few respiratory quotients actually measured suggested that the effects of the light supper (consisting mostly of carbohydrates) had passed off.[39]

The question then arises—Is sleep *per se* a depressing factor of such moment? The Nutrition Laboratory has accumulated a rather large amount of data on subjects asleep and awake. This is not the place to discuss the data, as the research is still in progress (necessarily very slow, as the opportunity for such measurements occurs only rarely). The picture thus far is by no means uniform. As regards the Benedict and Talbot series of girls from 8 to 12 years of age, who were studied in the New England Home for Little Wanderers, very careful records were kept with regard to whether the girls were awake or asleep in the clinical respiration chamber. On a number of the curves which we prepared in con-

[38] Krogh, "The Respiratory Exchange of Animals and Man," London, 1916, p. 56.

[39] A very recent and careful study by Professor Blunt (*Journ. Biol. Chem.*, 1924, *59*, p. 77) supports this conclusion.

nection with this study we indicated those girls who were awake through the entire experiment, those who were asleep throughout the entire experiment, and those who were both awake and asleep. It was subsequently found that in this age range from 8 to 12 years essentially the same number of points for girls awake or asleep or partly awake and asleep were each side of the line indicating the general trend of metabolism. These data, therefore, seemed to indicate that the effect of sleep was negligible. But, on the other hand, the few Girl Scout periods available suggested a profound decrease in metabolism during sleep.

Although the question of the influence of sleep is still held in abeyance, it seems unwise to raise the standards arbitrarily, but of course this could be done. In the belief, however, that the metabolism would best be expressed as near basal as possible, a series of recommendations for the prediction of the metabolism of females from birth to full maturity is here given, based upon these studies with girls, women, and indeed infants.

There has been no new contribution to the data on the metabolism of new-born infants since the extensive series of measurements by Benedict and Talbot, and hence the method of prediction recommended by them[40] is still the best in existence. It was found that there was an intimate relationship between the heat production and the length and a formula was devised to take into consideration both the length and the body surface. This formula, which is applicable to both males and females, is as follows:

$$h = l \times 12.65 \times 10.3 \sqrt[3]{w^2}$$

in which h equals the total heat production per 24 hours, l equals the length in centimeters, and w the weight without clothing, in grams, while 12.65 is a constant representing the average calories per square meter per 24 hours per centimeter of length and $10.3 \sqrt[3]{w^2}$ represents the body surface of the child. Owing to the very unstable heat regulation of the new-born infant during the first day of life, Benedict and Talbot suggest that no predictions can be made with any degree of accuracy until the child is 1½ days old, and this formula is recommended for children from 1½ to 6 days old.

[40] Benedict and Talbot, Carnegie Inst. Wash. Pub. No. 233, 1915, p. 108.

After 6 days of age and up to 12 years (*i.e.*, from 48 to 138 centimeters in height) it is recommended that the predictions be made for girls on the basis of the curve for the total heat production referred to height (see Fig. 1). Accordingly in Table V. we have given the basal 24-hour heat production predicted from height for girls in this age range, that is, with heights from 48 to 138 centimeters.

TABLE V.

BASAL 24-HOUR HEAT PRODUCTION OF GIRLS FROM THE FIRST WEEK AFTER BIRTH TO 12 YEARS OF AGE, PREDICTED FROM HEIGHT.

Height.	Predicted Heat.	Height.	Predicted Heat.	Height.	Predicted Heat.	Height.	Predicted Heat.
cms.	cals.	cms.	cals.	cms.	cals.	cms.	cals.
48	122	71	483	94	630	117	837
49	136	72	500	95	637	118	847
50	150	73	516	96	644	119	857
51	165	74	530	97	651	120	866
52	178	75	543	98	659	121	875
53	194	76	557	99	667	122	885
54	208	77	567	100	675	123	894
55	222	78	575	101	685	124	904
56	236	79	583	102	693	125	915
57	250	80	586	103	700	126	925
58	268	81	591	104	711	127	935
59	283	82	595	105	720	128	945
60	300	83	598	106	730	129	956
61	318	84	602	107	740	130	965
62	332	85	605	108	749	131	975
63	350	86	607	109	759	132	985
64	367	87	610	110	769	133	995
65	384	88	612	111	778	134	1005
66	401	89	615	112	788	135	1016
67	418	90	617	113	797	136	1026
68	435	91	620	114	807	137	1037
69	452	92	623	115	817	138	1047
70	468	93	626	116	828		

For girls from 12 to 20 years it has been shown that this heat-height relationship does not hold, and hence it is recommended that for this age range the predictions be based upon the relationship between the heat production per kilogram of body weight and age, as illustrated in Fig. 3 and tabulated in Table VI.[41] While this

[41] Previously published by Benedict, *Boston Med. and Surg. Journ.*, 1923, *188*, Fig. 2, p. 134, and Table V, p. 137.

recommendation is based exclusively upon the Girl Scout work, it is important to note that the Girl Scout work did not include the range between 18 and 20 years, and we have therefore projected the curve in Fig. 3 as a straight line between 18 and 21 years.

TABLE VI.

BASAL HEAT PRODUCTION PER KILOGRAM OF BODY WEIGHT PER 24 HOURS PREDICTED FROM AGE, FOR GIRLS 12 TO 20 YEARS OF AGE

Age.	Predicted Heat.	Age.	Predicted Heat.
yrs.	cals.	yrs.	cals.
12	31.0	16	21.9
12½	29.8	16½	21.8
13	28.6	17	21.8
13½	27.4	17½	21.8
14	26.2	18	21.8
14½	25.0	18½	21.8
15	23.8	19	21.8
15½	22.6	19½	21.8
		20	21.8

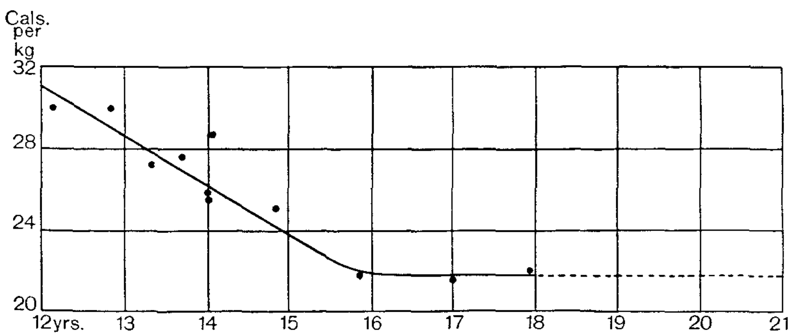

FIG. 3. Basal heat production of girls per kilogram of body weight per 24 hours referred to age.

For adult women 21 years of age and over the formula given by Harris and Benedict[42] is still recommended, that is,

$$h = +655.0955 + 9.5634w + 1.8496s - 4.6756a,$$

where h equals total heat production per 24 hours, w equals weight in kilograms, s equals stature in centimeters, and a equals age in years.

At this point it becomes of interest to compare the metabolism measurements made on other series of girls with those of the Nutri-

[42] Harris and Benedict, Carnegie Inst. Wash. Pub. No. 279, 1919, p. 227.

4

tion Laboratory series. The most extensive measurements with which we have become familiar are (1) the measurements of school children by Miss Bedale[43] of the University of Glasgow; (2) an unpublished series on girls (ranging in age from 8 to 18 years) made by Professor Katharine Blunt in the Department of Home Economics in the University of Chicago and most generously placed in our hands by Professor Blunt; and (3) an extensive unpublished series on girls (ranging from 10 to 15 years of age) made by Miss Grace MacLeod in the Department of Nutrition, Teachers College, Columbia University, New York City, access to which data has very kindly been given to us by Miss MacLeod. How did the metabolism measurements of these girls compare with the Nutrition Laboratory predictions for the total heat production referred to height and how did they compare with the Girl Scout measurements?

The data for the girls measured by Miss Bedale, as originally published, are grouped in such a way that individual values do not appear, but Miss Bedale has very kindly sent us the individual data. In so doing she has likewise written us a very frank statement to the effect that there are wide variations in her data that she herself is quite unable to explain. On the assumption that the *lowest* metabolism values represent basal and that all others are higher than basal, we have selected from Miss Bedale's individual data the lowest value for each of her girls. On this basis of selection 30 measurements are available for comparison with the prediction methods recommended. Of these selected minimum values only 7 can be compared with the Nutrition Laboratory prediction curve for the total heat production referred to height, since most of her girls are over 138 centimeters in height (beyond which height the curve is not recommended for predictions). Of these 7 measurements 3 are above and 4 below the prediction curve. When the metabolism is predicted on the basis of the total heat production divided by the pelidisi referred to height (see Fig. 2) and also on the basis of the heat production per kilogram of body weight referred to age (see Fig. 3), it is found that of the 30 selected values two thirds are above the predicted and one third below. To this comparison there are, however, two objections, which only those

[43] Bedale, *Proc. Roy. Soc., London,* 1923, Ser. B, *94*, p. 368.

who have seen the original data could raise. In the first place, there are a few values in the original data that must, judging from all practical experience, represent either leaks in the mask or incomplete collection of expired air which would result in an extremely low metabolism, a metabolism that perhaps on the next day, under better conditions, might be nearer that normally found with children. If one ruled out these few very low values and could have available the measurements secured under correct conditions, this would tend possibly to bring above the prediction line some of the values now so low. In the second place, a not inconsiderable number of girls were measured only once in one period, although Miss Bedale had taken the precaution to give them some prior experience with the routine. All previous experience in metabolism work indicates that the metabolism measured during the first period is apt to be high because of anxiety or apprehension, while subsequent periods will usually show a lower metabolism. Some of the values found above the Nutrition Laboratory prediction curve might on this basis be lowered and more closely approach the predicted metabolism. This is not the place to discuss in detail Miss Bedale's exceptionally well designed study, but these two criticisms would tend to raise the very low values and lower some of the high values, so that on the average these girls would probably have a metabolism not far from the predicted metabolism on the three bases under consideration.

When we plot the data for Miss Blunt's measurements on 56 girls on the basis of the total heat production referred to height and compare this chart with the curve given in Fig. 1 for the heat-height relationship noted with the Nutrition Laboratory series of girls, we find that the scatter of the plotted points is perceptibly above the Nutrition Laboratory curve, roughly speaking about 10 per cent. Similarly, when compared with the prediction curve for the total heat production divided by the pelidisi referred to height and with the prediction curve for the heat production per kilogram of body weight referred to age, her values lie on the average about 10 per cent. above the prediction curves.

The data for Miss MacLeod's girls cannot be compared with the predicted values from the heat-height relationship, since all but

three of her girls are over 138 centimeters in height, beyond which point the prediction on this basis does not go. When compared with the predicted values involving the pelidisi, her values show the same picture as the Blunt data, namely, they are on the average about 10 per cent. higher than the predicted values. As a test of the method of prediction recommended for girls from 12 to 20 years of age, namely, on the basis of the heat production per kilogram of body weight referred to age (see Fig. 3 and Table VI.), we have computed average values for the metabolism per kilogram of body weight from the data placed in our hands by Miss MacLeod. With her 12-year-old girls the average of 30 observations shows that the heat production per kilogram of body weight per 24 hours was 32.6 calories, a value approximately 5 per cent. higher than that predicted from the Girl Scout smoothed curve. Her 38 measurements on girls 13 years of age give an average value of 31.2 calories, or a value about 10 per cent. above the prediction from the Girl Scout smoothed curve. Her 17 observations on 14-year-old girls give an average value of 26.9 calories, which is practically identical with that predicted from the Girl Scout curve.

The final outcome of these comparisons, therefore, is that the Bedale data, with certain criticisms taken into consideration, agree fairly well with the Nutrition Laboratory predictions, but that both the Blunt and the MacLeod series indicate a general trend of metabolism about 10 per cent. higher, save that on the basis of the heat production per kilogram of body weight the 12-year-old group and the 14-year-old group of the MacLeod series show essentially the same values as the Girl Scouts. It is chiefly in the age range from 8 to 18 years that the data of these two investigators can be compared with the Nutrition Laboratory series, and the greater portion of this age period with the Nutrition Laboratory series is represented by the Girl Scout data, which have been considered by a number of writers as too low. The straight line pelidisi relationship for the entire age range represented by the Nutrition Laboratory series makes it difficult, however, to conceive that the Girl Scout data are too low for basal measurements. As has already been pointed out, it may be argued that the Girl Scouts were measured during deep sleep and hence their basal metabolism measurements may be low.

But from the fact that in this straight line pelidisi relationship the data for the adult women represent measurements obtained in practically all cases when the women were awake and that the girls younger than 12 years (studied in the respiration chamber by Benedict and Talbot) represent equal groupings above and below the central line of those who were awake and those who were asleep, it is difficult to assume that sleep alone can account for the discrepancy between the Nutrition Laboratory series and the MacLeod and Blunt series. This straight line function, therefore, when the pelidisi is brought into consideration, has rather an important bearing in throwing light upon the question as to the legality of using the Girl Scout data. If the Girl Scout data are too low and must be raised, then obviously the straight line function ceases, and in this discussion one must bear in mind that we are considering the straight line function chiefly as a physiological relationship extending over a period from 3 months to old age, rather than perhaps as the most ideal prediction method.

On the basis of this straight line function with reference to the pelidisi, therefore, we are forced to the belief that there was some factor entering into the measurements of the MacLeod and Blunt groups that tended to increase their basal metabolism. A personal visit to each of these laboratories has convinced the writer that the technique of both investigators is all that could be desired, and it is quite clear that we cannot explain this difference on the basis of a systematic error. The method of testing the apparatus for tightness by placing a weight upon the spirometer before the experiment begins, and particularly the method of testing the apparatus for leaks during the experiment by placing a weight on the spirometer when the experiment is half finished, rules out any possibility of a leak, although it is necessary to point out that a leak in the apparatus, if it existed, would invariably increase the apparent absorption of oxygen and thus explain in part this discrepancy. The writer does not believe that either of these apparatus leaked or that the investigators' technique was in any way at fault.

All conceivable explanations for the discrepancy between the Nutrition Laboratory measurements and the Chicago and New York measurements have been carefully gone over with Professor Blunt

and Miss MacLeod, and the influence of a hitherto disregarded factor appeared to be possible. The girls studied by the Nutrition Laboratory, particularly the Girl Scouts, were measured after having been in bed for many hours while they were warm, quiescent, and in the case of the Girl Scouts in deep sleep. The girls studied by Miss Blunt and Miss MacLeod had to leave their homes and go to the respective laboratories (in many instances, it is true, not great distances), which may have involved more or less stair climbing. They then lay down upon a couch the prescribed length of time, *i.e.*, half an hour before the beginning of the experiment. Miss Bedale's girls, on the other hand, were measured more in conformity with the Nutrition Laboratory Girl Scout series, that is, they were studied in a school dormitory and the apparatus was taken to the bedside. The children were therefore warm and thoroughly relaxed, and did not have to get up, dress themselves, wash, go outdoors in possibly inclement weather, and then ascend the stairs in another building, all prior to the metabolism experiment. The possible influence of this activity between the hour of arising in the morning and the beginning of the metabolism experiment has heretofore been entirely ignored. Whether the effect is proportional to the temperature outdoors or the inclemency of the weather, the distance walked or the condition of the terrain, is very difficult to state with the present data at hand. We have gone over an extensive series of data with a subject who has come to the Nutrition Laboratory morning after morning for many years. We have drawn off the data for those days when the walking was very bad, the temperature low, and the wind severe, and frankly, we cannot find anything in the data from this one adult to confirm this suggested possible explanation of the difference in the metabolism measurements of the several series of girls. A further source of perplexity is the fact that the 14-year-old group of girls studied by Miss MacLeod give an average value for the heat production per kilogram of body weight which is practically identical with that predicted from the Girl Scout curve (see Fig. 3), while the 12-year-old girls give an average value only 5 per cent. above, though the rather large 13-year-old group show definitely a metabolism 10 per cent. above the Nutrition Laboratory series.

As stated elsewhere in this paper, one of the most important

features in the measurement of basal metabolism is the comparison of the metabolism of a pathological case with what is supposedly normal. If, therefore, a girl is measured under ordinary conditions in the clinic or doctor's office, where prior to the measurements she has had to go through the exercise involved in arising, dressing, and getting to the laboratory, and if under these conditions she has a metabolism that is 10 per cent. above the Nutrition Laboratory standard, it is quite clear that, based upon the Chicago and New York work, this can be wholly disregarded. What is to be the case if the girl shows a metabolism 15 or 20 per cent. above? Certainly no one would justify a diagnosis of an abnormal metabolism on this basis. The satisfactory procedure would be to have the girl spend the night in the hospital or, indeed, her home and measure her metabolism in the morning immediately before arising.[44] On the other hand, if the metabolism is found to be appreciably lower than the Nutrition Laboratory standard under the conditions outlined above, it is quite likely that one is dealing with a girl who is somewhat below par. In clinical measurements, when the subject comes to the office after a walk or after the general activities incidental to getting up, dressing, etc., the metabolism may be distinctly higher[45] than that measured in bed when the subject is quiet and asleep, under which latter condition more nearly the true basal value would be obtained. Our recommendation would be, therefore, that in such measurements as are made in the office or the clinical laboratory in the hospital one must recognize the possibility of a difference in metabolism.

In these predictions it is obvious that the correlation between height, weight, and age (which is normally found with children) obtains. It is quite clear that with children of excessive weight, with marked obesity, the prediction curve involving only height will not predict what is actually found. It is again to be questioned whether such children do not come under the head of pathological cases. The Nutrition Laboratory has maintained that the basal

[44] With the new student form of respiration apparatus (described by Benedict and Benedict, *Boston Med. and Surg. Journ.*, 1923, *188*, p. 567) the procedure is thoroughly practical, as the apparatus is completely portable, needing only to be supplemented by a small cylinder of oxygen.

[45] This factor should be immediately investigated and reported by as many as possible of the army of clinicians now equipped with the newer forms of respiration apparatus.

metabolism of the normal child is first to be known and should be the standard of reference for all others. If allowances are to be made for excessive weight or for decreased weight, such allowances can come later with a better understanding of the subject. Consequently, while we freely admit that other methods of prediction may be (for the time being, at least) of more help to the clinician and more practical from his standpoint, we still feel that the closest approximation to the real basal metabolism is that which should be reported. Hence in this connection we have given our recommendations for the closest approximation to basal, frankly stating that, if girls are found to be 10 per cent. above this standard, the work in the New York and Chicago laboratories definitely proves that, measured under the conditions ordinarily obtaining, this increase of 10 per cent. is without any significance whatsoever. If the increase is found to be 20 per cent. above basal, before concluding that the metabolism represents a pathological condition, the child should be studied after a night's repose in bed.

Summary.

Based upon an analysis of the Nutrition Laboratory series of measurements of infants, young children, Girl Scouts, and adult women, it is found that a most important factor correlating with basal metabolism measurements is height. The importance of introducing some index of the state of nutrition, such as Pirquet's pelidisi, and the fact that the total 24-hour basal heat production divided by the pelidiši referred to height shows a straight line relationship from 3 months to adult life further justifies the emphasis laid upon the height element in basal metabolism predictions. For new-born infants, both males and females, from 1½ to 6 days old it is recommended that the formula involving length, previously published by Benedict and Talbot, be employed in predicting the metabolism. For girls from 1 week to 12 years of age a new method of prediction involving height alone is recommended. From 12 to 20 years of age the metabolism of girls may at present be best predicted from the curve based upon a series of experiments with Girl Scouts, showing the heat production per kilogram of body weight per 24 hours referred to age. For adult women 21 years and over the formula of Harris and Benedict for women, involving age, weight, and height, is retained.

SOME NEW EXPERIMENTS IN GRAVITATION.

FOURTH PAPER.

By CHARLES F. BRUSH.

(*Read April 26, 1924.*)

The writer outlined "A Kinetic Theory of Gravitation" before the American Association For The Advancement of Science in December, 1910.[1] Since that time he has presented several papers on the subject of gravitation,[2] the last one at the general meeting of this Society in April, 1923.

That paper was chiefly descriptive of apparatus and method for comparing the velocities of freely falling bodies in two aluminum containers, alike in size, shape, and smoothness of surface, and dropped *simultaneously*, side by side, through *exactly* the same distance (about 122 cm.). The description was freely illustrated with plates and diagrams.

Each container, at the end of its journey, breaks an electric circuit. But the breaks of both containers are in series in the same circuit, so that the break which occurs first produces a bright spark, while the belated break gives no spark because its circuit is already open.

When the containers are equally loaded with the same metal, there is no visible spark at either break or a very feeble spark at one or the other indifferently. But when they are equally loaded with certain different metals, one container persistently produces a bright spark, though the containers are always reversed in position for each trial. From this it seems clear that the container giving the spark falls a little faster than the other. This sparking condition is clearly manifested when the faster container reaches the end of its free path as little as .0125 mm. (.0005 inch) in advance of its neighbor. This indicates a time difference about 1/400,000 second. (During their half second of falling the containers acquire a velocity about 4900 cm. per second).

[1] *Science*, March 10, 1911; *Nature*, March 23, 1911.

[2] *Proc. Am. Phil. Soc.*, Vol. LIII., No. 213, January-May, 1914; Vol. LX., No 2, 1921; Vol. LXI., No. 3, 1922; Vol. LXII., No. 3, 1923.

Some of the metals show very many times greater difference in falling velocities than the near minimum indicated above (about 1/100,000) and the paper referred to describes a method of approximate measurement of the lag of the slower container.

Following the presentation of the last paper referred to, many comparisons of metals and other substances were made with every precaution that could be thought of to avoid error, and in the light of much experience with the apparatus. The general results are embodied in the "Table of Relative Falling Velocities" herewith.

TABLE OF RELATIVE FALLING VELOCITIES.

Fast.	Platinum.
	Gold coin, Tungsten forged, Lead.
	Bismuth.
	Zinc, Tin.
	Aluminum, Magnesium, Carbon, Silicon, Boron, Water.
Slow.	Sulphur, Selenium.

The spacing of the lines indicates approximately the relative velocity differences found. Differences, if any, between substances arranged in the same line were too small to be appreciable.

The observed difference in the falling velocities of platinum and sulphur (lag of the sulphur) is approximately .02 cm. in the 122 cm. of fall; or about 1 part in 6000.

The quantitative value of zinc in the table is somewhat uncertain, for reasons indicated in former papers, and later in the present paper.

It seems significant that the fast-falling metals, platinum, gold, tungsten and lead all have high atomic weight, and density. Lead, though less dense than the others, has higher atomic weight. Yet bismuth, with virtually the same atomic weight as lead, and little less density, is considerably slower.

Much lower down in the scale of velocities we find the elements of comparatively very low atomic weights and small densities, aluminum, magnesium, carbon, silicon, boron (and water) with no appreciable difference in behavior. This also seems significant.

Sulphur and selenium are in a class by themselves, being much slower than the light elements last referred to. Much care was taken to make sure of this. They were always melted in their containers, and each was checked repeatedly, and very satisfactorily, with aluminum and carbon.

It was rather expected that tellurium (also melted in its container) would, on account of its chemical similarity, behave much like sulphur and selenium. But it did nothing of the sort. On the contrary it behaved very much like zinc and tin. Probably this was due to its comparatively high atomic weight and density. This again seems significant.

In general then, it appears that the metals of very high atomic weight and great density have the highest falling velocities. And elements of very low atomic weight and small density have the lowest falling velocities; while elements of intermediate atomic weights and densities (zinc, tin and tellurium) have intermediate falling velocities or mass-weight ratios.

Allotropic condition seems not to affect mass-weight ratio. Thus, Acheson graphite and electric light carbon showed no difference in falling velocities. And the same was true of ordinary yellow sulphur and its gutta percha-like modification obtained by melting at a much higher temperature; though much heat energy must have been absorbed and rendered latent during the change.

In a former paper it was shown that "fusible alloy" behaved the same as its constituent metals mixed but not alloyed.

Only one experiment has been made involving chemical combination. Lead sulphide (galena) was found a little slower than metallic lead. As nearly as could be estimated, it behaved like a

mixture of lead and sulphur in the proper proportions to form the sulphide. Apparently, in this case, chemical combination does not affect mass-weight ratio. The writer thinks this is probably true as a general proposition, though of course a single isolated experiment is but a small step toward establishing it.

In the hope that some close relationship might be found between the observed falling-velocities of the elements tested, and their physical or chemical properties or combinations of properties, a table of all the common elements was prepared, arranged in the order of their densities. In parallel columns were placed the corresponding atomic weights, atomic numbers, specific heats and thermal conductivities both in terms of weight and volume. But a careful study of this Table failed to shed any new light on the subject.

It was thought that something of value might be learned by comparing the transparency (or opacity) to very hard X-rays of equal weight-thicknesses of some elements. To this end discs or cylinders of 24 elements, including all those shown in the Table except boron, were prepared of thicknesses inversely proportional to their densities; so that the X-rays must pass through equal weights of the several elements before reaching the photographic plate below them. Thus the platinum disc was only .117 cm. thick, while the magnesium cylinder was 1.439 cm. high. All were mounted in a circle of holes in an aluminum plate or carrier, placed directly over the photographic plate with only the usual orange and black papers intervening. The photographic plate was backed by a thick lead plate, and the whole outfit was revolved in a horizontal plane by clockwork, around the center of the circle of specimens; thus securing equal exposure to all. Several revolutions were made during an exposure.

The hard X-rays were furnished by a Coolidge tube with tungsten target, and excited by 200 Kv. The target was placed 1 meter above the photographic plate, and the rays were filtered through a 3/4 mm. copper plate and an aluminum plate. 15 or 20 seconds exposure was found suitable.

Prints from the negatives were made through a wide range of exposures under a tungsten lamp, while the negatives were revolved as before, to secure uniform exposure.

The prints are beautiful and extremely interesting in several ways;

but while they show strong contrast between the platinum and aluminum groups, they do not disclose anything more specific.

Toward the close of last year's paper the writer cited reasons for believing that the mass-weight ratios of some metals, particularly bismuth and zinc, are not constant, but vary slightly with physical condition. In other words, that a constant mass, or quantity of metal may be appreciably changed in weight by changing its physical condition.

The writer has been earnestly at work on this problem for six months or more, and occasionally long prior to that, principally endeavoring to find a practicable method of attack; and has so far succeeded as to leave very little room for doubt that actual weight changes are being effected. The observed effects, though small, are several times larger than the probable experimental error.

The best method of procedure thus far found consists in certain heat-treatments of the metal, to bring about a definite molecular condition, before the first weighing. This is essential. After weighing, the metal is subjected to more or less drastic mechanical treatment, usually in several stages, with occasional periods of rest to permit the spontaneous changes which sometimes occur, and reweighed after each stage of treatment. The treatments are so conducted as to preclude any loss or gain of substance.

The refinements of weighing and precautions against error, developed through many years of experience in refined weighing, will not be detailed here but reserved for a future paper, after this fascinating subject of weight-change has been more fully exploited.

CLEVELAND, OHIO.
April, 1924.

THE AMENDING PROVISION OF THE FEDERAL CONSTITUTION IN PRACTICE.

By HERMAN V. AMES.

(Read April 24, 1924.)

The constitutional developments during the last decade have directed more attention to the amending process of the Federal Constitution than at any time since the adoption of the three reconstruction amendments, during the years 1865–1870.

The successive addition of four amendments, the sixteenth to the nineteenth, within a period of scarcely more than ten years (1909–20), after an interval of some forty, during which no amendments had been secured, in itself is sufficient to challenge attention. But when we consider the fact that at least three of these four amendments have affected profoundly the private and public life of the people, and recall the agitation accompanying their proposal and ratification, as well as the difficulties encountered in enforcing the eighteenth amendment and the laws made in pursuance thereof, we begin to realize some of the factors that have combined to make the subject of the amending power a problem of vital interest.

Moreover, several important constitutional questions of procedure in connection with these recent amendments, especially the eighteenth and nineteenth, have been judicially determined by the federal Supreme Court, and this has led to attention being focused on the provisions of Article V. This Article, it will be recalled, provides that amendments may be proposed either by a two-thirds vote of both Houses of Congress or by a Convention called upon the application of two thirds of the state legislatures, which in either case shall be valid when ratified by the legislatures of or by Conventions in three fourths of the several states, as Congress may direct.

It is a noteworthy fact that only the first method of proposing amendments has been used, and that Congress always has prescribed the method of ratification by the state legislatures.[1]

[1] Ames, "Proposed Amendments," 290. (See note 2.)

A review of our history since the Constitution went into force shows that although the number of amendments actually effected have been few, only nineteen, there has been no lack of effort to secure many others.

A study made by the speaker many years ago, of the attempts to amend the Constitution during the first century of its history, revealed the fact that over thirteen hundred distinct resolutions, proposing over nineteen hundred amendments, had been presented in Congress.[2] Dr. Jacob Tanger, formerly a graduate student at the University of Pennsylvania, now connected with the Faculty of Pennsylvania State College, at my suggestion undertook a similar study of the period since March 4, 1889. He has completed a survey of the records to March 4, 1923. I am indebted to him for the statistics for the period.[3] In these thirty-four years over eleven hundred resolutions had been proposed, suggesting over sixteen hundred amendments, or a total for a century and one third, of more than thirty-five hundred amendments. The small chance of success doubtless has prevented the number being even larger.

As is familiar to all, only fifteen amendments were secured during the first century of our history under the Constitution, the first ten coming so soon after the adoption of the Constitution as to be practically contemporary with it. The eleventh and twelfth were

On a few occasions during the first century of the history of the Constitution efforts were made to secure the calling of a Constitutional Convention to propose amendments, notably, just before and during the Civil War period, and on two occasions thereafter. A review of the efforts to call a convention to propose amendments and also the resolutions to cause their submission to conventions in the states instead of to the legislatures to 1889 will be found in Ames, "Proposed Amendments," 281–284, 286–287.

In more recent years six states, chiefly Western, have memorialized Congress for such a general convention, while a number of others have requested the calling of such a body to propose an amendment on some particular subject. Some individuals or political groups that wished a fundamental revision of the Constitution, as the Socialist Party, have also advocated a general convention

[2] Herman V. Ames, "The Proposed Amendments to the Constitution of the United States during the First Century of its History," Annual Reports of the American Historical Association for 1896, Vol. II., pp. 442, Washington, 1897.

[3] Monograph not yet published, also J. Tanger, "Amending Procedure of the Federal Constitution," *American Political Science Review*, X., 689–699. Michael Angelo Mussman, "Is the Amendment Process too Difficult?" *American Law Review*, LVII., 694–705.

added within the next few years. Thus, the first twelve were secured in the first fifteen years or by 1804. No further amendments were obtained during the succeeding sixty years when the thirteenth, fourteenth and fifteenth were adopted in the five years from 1865 to 1870, registering the results of the war.

Then ensued a period of over forty years before another amendment was adopted, but in the following decade, 1909–20, the last four amendments, the sixteenth to nineteenth, were ratified.

Congress proposed four other amendments that were not ratified, two in 1789, one in regard to the apportionment of representatives and one relative to compensation of members of Congress. The third, prohibiting the acceptance of titles of nobility, was referred to the state legislatures in 1810 and the fourth proposal in 1861 was intended to prevent the secession of the Southern States, prohibiting any amendment abolishing or interfering with slavery within any state. Two of these four, the first and the third mentioned, only lacked the ratification of the legislature of one state to have secured their adoption.[4]

In addition to the above, eighteen other proposals were approved by one branch of Congress during the first century, nine in the Senate and nine in the House.[5] During the first third of the second century under the Constitution, Dr. Tanger's examination shows that fourteen have passed one branch of Congress, an equal number in the House and Senate.

Thus, during the entire period of one hundred and thirty-four years, to March 4, 1923, thirty-two resolutions proposing amendments have passed one branch of Congress and failed of submission through the lack of the necessary approval of the other legislative chamber.

In the present Congress one other resolution, proposed by the Senate in 1923, has again been endorsed by the same body and has received a favorable report from the House Committee. This provides for a change in the date for the convening of Congress, and the inauguration of President and Vice-President to the first and third Mondays in January, respectively, following the elections.[6]

[4] Ames, "Proposed Amendments," 34, 43, 44, 186–189, 195–197, 200.

[5] Ibid., 300.

[6] Vote of 63 to 6, February 13, 1923; Senate Report, No. 933, M. A. Mussman, *American Political Science Review*, XVIII., 115–118.

It is quite possible that this may become the twentieth amendment, as there is little or no opposition to the proposed change, which is regarded generally as most desirable for reasons that are so obvious that time will not be taken to enumerate them.[7] Another amendment which was endorsed by the House last year provided for the taxation of state and federal securities.[8]

A review of the proposals made to amend the Constitution during the first third of its second century is very illuminating. Many of these, like those of the first century, reflect contemporary problems and serve as indices to some of the leading issues of the day. Many others are similar to those presented in the earlier period, and are indicative of the popular dissatisfaction with certain features of the Constitution and the recurrence or persistence in the demand for their change.

Typical of this class are proposals to change the method of electing the President and the Vice-President, and especially the direct election of Senators by the electors. This latter amendment was first proposed in 1826. It was renewed from time to time in each of the following decades until 1872. Subsequent to that date scarcely a session of Congress passed without its being urged. In the period of eighty-six years since its first introduction it was proposed nearly two hundred times (198). Five times it passed the House, beginning with the fifty-second Congress, only to be blocked in the Senate, but finally, in response to the popular demand, the Senate yielded in 1912 and the ratification of the seventeenth amendment followed the next year.[9]

Various other proposals to change the form of the government in some of its features have been presented, but by far the most numerous amendments suggested related to the powers of the Federal Government, chiefly efforts to extend its scope, the sixteenth, eighteenth, and nineteenth amendments being the successful ones.

In consequence of the adoption of these recent amendments, a decided impetus has been given to the attempts to obtain still further

[7] Ibid.

Congressional Record, 67th Congress, 4th Session, 704, 709–729, 2253–2284.

Ames 61, 62.

[8] January 23, 1923.

[9] Tanger, *American Political Science Review,* X., 696–698.

changes in the Constitution. Thus in the last Congress, the sixty-seventh Congress, one hundred and three joint resolutions were introduced on over two score different subjects (44), ten of these to change the method of amendment.[10] In the opening days of the present Congress, last December, fifty-nine proposed amendments were presented.[11] Many of these relate to new problems suggested for example by conditions arising out of the recent war, the development of trusts and monopolies, as well as many relating to a variety of financial, commercial, territorial and miscellaneous matters.[12]

The decision of the United States Supreme Court pronouncing certain laws unconstitutional has led to various proposals to regulate the exercise of this power by the Judiciary.[13] The decision of the Supreme Court in 1922 declaring the Child Labor Law of February 24, 1919, unconstitutional[14] led to the immediate introduction into the Congress then in session of over a score of joint resolutions to confer the power in question on Congress. At the opening of the present session of Congress seventeen similar proposals were introduced and it is highly probable that Congress will submit such a proposed amendment to the State Legislatures for ratification in the near future.*

It is rather surprising, in view of the large number of amendments introduced in Congress and the few secured during the first century, that no suggestion was made to change the method of amendment until three quarters of the century had passed; it was not until the Civil War period that the first was presented. In 1864 a resolution was proposed for the reduction of the majorities required both for the proposal and ratification of amendments.[15]

[10] *Congressional Digest,* March, 1923, 172.

[11] *Congressional Record* to December 15, 1923.

[12] For an enumeration of these proposals see *Congressional Digest,* March, 1923, 172; *The American Political Science Review,* XVIII., 88–91.

[13] For a review of the proposed amendments to curb the power of the courts see *Congressional Digest,* June, 1923, 271.

[14] Child Labor Tax Case, 259, U. S. 20.

* Since this was written Congress, on June 2, 1924, has submitted to the legislatures of the States the following proposal: "The Congress shall have power to limit, regulate and prohibit the labor of persons under eighteen years of age."

[15] Ames, 292–293.

At the time the joint resolutions which later became the fourteenth and fifteenth amendments were pending, several members of the Democratic Party attempted to require their submission to legislatures of which at least its most popular branch must have been elected subsequent to the submission of the amendment. A similar provision was revived in 1882.[16] These foreshadowed some of the proposals of the present day.

Only a few other suggestions to alter Article V. were presented during the last quarter of the first century. Senator Morton (1882) brought forward a resolution to prescribe rules to be followed by the legislatures when acting on amendment proposals.[17] Two others are noteworthy as proposing the ratification of amendments by the vote of the people instead of the legislatures, thus anticipating the referendum system urged so frequently in recent years.[18]

While few formal proposals were made in Congress to change the method prescribed for amending the Constitution prior to the opening of the present century, there was a growing opinion among jurists and publicists during the closing years of the nineteenth century that the amending process was too difficult. Even at a much earlier day Chief Justice Marshall had characterized the machinery for amending the Constitution as "unwieldy and cumbrous."[19]

Woodrow Wilson, in his first important work, "Congressional Government," published in 1885, wrote: "It would seem that no impulse short of the impulse of self-preservation, no force less than the force of revolution, can nowadays be expected to move the cumbrous machinery of Article V."[20] Yet later, while President, he was to witness four amendments adopted without any upheaval through the normal working of the provisions of this Article.

A speaker at the meeting of the American Bar Association in 1907 even went farther when he declared that "if the Constitution is to be changed it must be done in the manner in which the instrument prescribes," which is "to say that it shall not be changed at

[16] Ibid., 288–290; Senator Ashurst reviewed some of these early attempts, *Congressional Record*, 67th Congress, 4th Session, 3498–3502.

[17] Ibid., 290–291.

[18] Ibid., 293–294.

[19] Baron *vs.* Baltimore, 7 Peters 761.

[20] "Congressional Government," 242–243.

all, for we are taught by a century of our history that the Constitution can no longer be amended," and so it seemed to many other jurists and historical students.[21]

To cite one other instance, I quote the words of the late Justice William P. Potter of the Supreme Court of Pennsylvania, contained in an address he delivered in 1909. "The expectation of the Fathers of the Republic that the method of amendment which they provided would be satisfactory has not been fulfilled," therefore, he thought it was "time for the American people seriously to consider the adoption of an easier process of altering or amending the Constitution." To this end he advocated the adoption of the system in practice in many of the states, namely that an amendment might be proposed by a majority vote of both houses of Congress and ratified by a majority of the qualified voters in a majortiy of the states.[22]

Several other proposals have been suggested by such eminent political scientists as Professors J. W. Burgess and Munroe Smith, designed to effect an easier method of amendment, for the details of which I must refer those interested to their published works.[23] Their plans are cited here simply to indicate that in the opinion of these distinguished experts a workable amending provision is of great importance.

The demand for an easier process of amendment found frequent expression in Congress in the years subsequent to 1909. Indeed, Dr. Tanger notes that since 1898 over two score joint resolutions have been introduced embodying some sixty changes in Article V. Down to 1919 the greater number of the changes sought aimed to secure an easier method of proposing amendments, some even providing for some form of popular initiative, as well as for a simpler method of ratification.

Typical of this class are the proposals advocated by Senator Cummins and LaFollette in 1913.[24] The latter's resolutions pro-

[21] Quoted in *University of Pennsylvania Law Review*, LVII, 589.

[22] Ibid.

[23] Burgess, "Political Science and Comparative Constitutional Law," I., 150–154 (1890); Munroe Smith, "Shall We Make Our Constitutions Flexible," 194, *North American Review*, 657.

[24] *Congressional Record*, 63d Congress, 2d Session, 1500, 1628, 5575; Senate Report, No. 147, Tanger, Opus cit., 699; W. F. Dodd, "Amending the Federal Constitution," *Yale Law Journal* (February, 1921), XXX., 351–352.

vided that amendments might be proposed in three different ways, either by a majority vote of both Houses of Congress, or on the application of the legislatures of ten states, or finally, on the application of ten states through the vote of a majority of the electors of each voting on the question. The proposals were to be submitted to the voters of the several states at the next ensuing election of Representatives, the same to be ratified if in a majority of the states a majority of the electors voting thereon approved.

Senator Owen repeatedly has proposed an even more varied plan for the initiation of amendments, coupled with a referendum to the voters, the amendment to be ratified if it secured a majority of the votes of those voting on the measure and also a majority of the votes of the congressional districts.[25]

A review of the facts shows that the ratification of amendments has been far less difficult than to effect their submission to the states. As already pointed out, only four of those submitted by Congress failed of ratification. Of those ratified, sixteen of the nineteen were ratified within fifteen months or less, the eleventh, fourteenth and sixteenth in a period from about two years to slightly less than four years. In one hundred and twenty-five years, or since 1810, only one amendment proposed by Congress failed of ratification and that was the amendment submitted in 1861 for the preservation of slavery. On the other hand, thirty-two resolutions, after passing one house, failed of submission owing to the lack of approval by the other branch of Congress.

The responsibility for the paucity of amendment is not due primarily to the lack of action by the states. The ease and speed with which the eighteenth and nineteenth amendments were secured has led to the belief since 1919 that the process of ratifying amendments is too easy, and in consequence the numerous proposals to change the provisions for securing amendments have placed the emphasis on the method of ratification. The proposals recently presented in general have aimed to prevent an amendment being ratified that does not have the prior approval of the people.

The shock that the Prohibition and Women's Suffrage amendments have given to the advocates of local autonomy has led them to advocate greater popular control in ratification, hence some form of

[25] *Cong. Record,* 66th Cong., 1st Sess., 5700.

referendum is provided for, as in the resolution submitted by Senators Brandegee and Walsh, or in the Wadsworth-Garrett proposal. The undemocratic nature of the existing method of ratification is now being dwelt upon. Some interesting statistics of the membership of the state legislatures have been compiled with a view of showing how small a number of legislators can control a majority in both branches of the legislatures of thirty-six states, or the three fourths majority necessary to ratify an amendment. These figures demonstrate that it is theoretically possible for 673 state senators and 1,643 representatives, or a total of 2,316 legislators, to do so, although this might be less than 32 per cent. of the total number of legislators in the forty-eight states. Further, it is shown that it would require only 167 votes in the Senates of thirteen of certain of the smaller states to prevent the repeal of an amendment.[26]

The proposal of Senator Brandegee which has been reported favorably twice from the Senate Committee of the Judiciary in recent Congresses leaves it optional with Congress to prescribe a third method of ratification, namely by the electors in three fourths of the states.[27]

The Wadsworth-Garrett plan, introduced both in the last and present Congress, has received much popular indorsement. Among other provisions it requires that the members of at least one House in each of the legislatures which may ratify shall be elected after the amendment has been proposed and further permits any state to require that ratification by the legislature be subject to confirmation by a popular vote.[28]

Senator Walsh's resolution, which recently has been reported favorably by the Senate Committee on the Judiciary as a substitute for the Wadsworth-Garrett plan, makes a referendum mandatory.[29] Both the legislatures of Louisiana (1920) and Texas (1924) have memorialized Congress to call a constitutional convention for the purpose of amending the Constitution so as to insure the submission of all amendments to the qualified voters.[30]

[26] *Cong. Record,* 67th Cong., 4th Sess., 5387–5388.

[27] *Cong. Record,* 66th Cong., 1st Sess., 5696–5700.

[28] *Cong. Record,* 67th Cong., 4th Sess., 2713, 5388. Hearing before the Subcommittee on the Judiciary of the Senate, Jan. 16, 1923, 16–17.

[29] Jan. 20, 1924, 68th Cong, 1st Sess., *Senate Reports,* No. 5, 6.

[30] *Cong. Record,* 66th Cong., 3d Sess, 31; Ibid, 68th Cong., 1st Sess. (Index, Texas).

This survey of the amending provisions in practice would be incomplete without a brief discussion of the more important of the decisions which the Supreme Court of the United States has been called upon to render since 1920, relating to several questions of procedure followed in the submission and ratification of amendments. These decisions have been given chiefly in connection with a series of cases growing out of attempts to have the eighteenth amendment declared void for alleged irregularities in some phase of the procedure followed.

In the National Prohibition Cases in 1920 [31] the Court held that the two-thirds vote in each house of Congress, which is required to propose an amendment, is a vote of two thirds of the members present—assuming the presence of a quorum—and not as contended a two-thirds vote of the entire membership present and absent. This decision was in harmony with the precedent established by Congress since early days, although it does not appear previously to have been determined judicially.

With the extension of the system of the referendum in the states on state laws and constitutions it was quite natural that the attempt should be made to render the same applicable to amendments to the Federal Constitution. In several states it has been held by State Courts that state referenda might be made applicable also to the federal amending process.

Ohio, however, is the only state having a constitutional provision explicitly requiring it, effected by an amendment to the Ohio Constitution in 1918. The validity of this requirement was considered in the case of Hawke *vs.* Smith, decided in 1920.[32] After an able discussion of the question of what constitutes the legislature of a state, the Court pronounced "the function of a State Legislature in ratifying a proposed amendment to the Federal Constitution" as "a federal function derived not from the people of that state, but from the Federal Constitution." Ratification therefore, "by the legislature of a state, is not an act of legislation, it is but an expression

[31] 253 U. S. 350. For a review of the Briefs presented in this and some of the following cases, see Dodd, *Yale Law Journal,* XXX., 321–354.

[32] 253 U. S. 221. J. I. Burnett, "From State to Federal Referendum," *American Law Review*, LV., 201–217 (1921); W. H. Taft, "Can Ratification of an Amendment to the Constitution be Made to Depend on a Referendum," *Yale Law Journal,* XXIX., 821.

of the assent of the state to the proposed amendment." The Court therefore held "the provision of the Ohio Constitution requiring a referendum as inconsistent with the Constitution of the United States." It would appear that the decision in this case prompted the introduction of the Wadsworth-Garrett amendment, already referred to, which would constitutionalize the referendum system.

In consequence of the ruling of the Court in the case of Hawke *vs.* Smith it is clear that the provisions of some of the other State Constitutions imposing limitations or conditions on the amending process, not found in the Federal Constitution, are ineffective. This would apply to the clauses in the Tennessee and Florida Constitutions that prohibit any convention or legislature of the state to act on any Federal Amendment unless such convention or legislature was elected subsequent to the submission of the amendment. As a matter of fact, the legislature of Tennessee ratified the nineteenth amendment without observing the aforesaid limitation of the State Constitution.

A further attempt was made to test the validity of the eighteenth amendment because of the provision in its third section requiring ratification within seven years from the date of its submission. This was the only instance where a definite period for ratification had been fixed. The opinion of the Supreme Court in 1921 in the case of Dillon *vs.* Glass [33] held that the question of "whether a definite period for ratification shall be fixed or not was . . . a matter of detail which Congress may determine as incident to its power to designate the mode of ratification." The Court showed that the question of limiting the period of ratification had been debated at length, and both houses of Congress had agreed that some limitation was intended and that seven years was a reasonable period. The Court referred to the fact that four amendments were outstanding and that three of them had been dormant for more than a century. Whether such an amendment could be resurrected and its ratification completed is still an open question. Eighty-four years after the partial ratification of one proposed in 1789 the Legislature of Ohio made an effort to complete its ratification but as no other state followed its example the matter was not a practical one.[34]

[33] 256 U. S. 368.
[34] Ames, 291–292.

In view of such a contingency as the one just mentioned, it would seem highly desirable that Congress should fix a limit to the period in which a proposed amendment is open for ratification. The proposal of Senator Brandegee, already discussed, fixed the limit to a period of six years.

In the case of Dillon *vs.* Glass the Court decided that the eighteenth amendment became a part of the Constitution on the date when the ratification of the states was consummated, and not on the date when the ratification was proclaimed by the Secretary of the State.

The question whether a state that has once ratified can withdraw its ratification first came up in connection with the ratification of the fourteenth and fifteenth amendments as three states attempted to do so in the case of the fourteenth amendment and one in the case of the fifteenth amendment.[35] In the former instance the Secretary of State being in doubt issued a certificate declaring the amendment adopted provided the previous ratification of the states attempting to withdraw the same was still to be considered valid. Congress immediately passed a concurrent resolution declaring the ratification of the fourteenth amendment valid and sufficient.

The latest attempt of this nature occurred in Tennessee, when the legislature, after a quorum had been broken, attempted to reconsider its previous ratification of the nineteenth amendment. There was no necessity of testing this action judicially as a sufficient number of other states ratified to secure the adoption of the amendment. A provision, however, in the Wadsworth-Garrett proposal covers such a contingency by permitting any state to change its vote until three fourths of the states have ratified, or more than one fourth have rejected or defeated the proposal in question.

In several of the briefs against the eighteenth amendment the Supreme Court was urged to place limitations upon the federal amending process, other than those stated in the Constitution, on the ground that the reserved sovereignty of the states was endangered by the ease with which amendments were being secured to extend the powers of the Federal Government.[36]

[35] Ibid., 299–300.

[36] Dodd, Opus cit.; Wm. P. Brynum, "State Rights and Federal Power," *American Law Review*, LV., 1–4 (1921); A. M. Holding, "Perils to be Apprehended from Amending the Constitution," *American Law Review*, LVII., 481 (1923).

In this connection it is of interest to note that whereas a few years ago some form of popular participation in the process of amending the Constitution was advocated by the so-called radicals, with a view of more easily securing amendments, today, on the other hand, the submission of proposed amendments to popular vote is urged by those who frankly avow that they expect it will render ratification more difficult. This is indicative of the distinct change of view that has taken place in the past ten years in consequence of the recent repeated demonstration of the effectiveness of the present amending provision of the Constitution.

Although the speaker some years ago held the view that the amending process was too difficult, he has been led, in common with others, as a result of recent experience, to a modification of that opinion. He believes that a radical change in the method of amendment is neither necessary nor desirable.

Possibly the amending machinery may need some modification. For example, the provision for the proposing of amendments might be made less difficult, as it has been shown that in the past the greatest obstacle experienced has been to secure the coöperation of both houses of Congress in submitting amendments, rather than their ratification by the state legislatures. Further, the proposal, common to several of the recent resolutions, that the amendment should be acted upon only by legislatures elected subsequent to its submission, as well as the limitation to a definite period during which the same is open to ratification, seem desirable regulations.

However, the speaker sees no necessity for a referendum to popular vote until at least a trial has been made of the optional method already provided for in the Constitution, namely, the ratification by conventions in the several states. It has been truly said that " the election of a convention would itself be a referendum," and by the submission of the proposed amendment to conventions especially chosen, the purpose of a referendum would be accomplished while avoiding its defects.

No change should be permitted that does not give full recognition to the federal principle in our system of government. The country, moreover, should be on its guard against a process of amendment of such ease that provisions might be introduced into

the Constitution which would establish as permanent features. passing political or sectional issues.

"Clearly," writes Dr. W. F. Dodd in an illuminating and comprehensive article, "the Federal Constitution performs a function different from that of the state constitutions, and should be less flexible than the state constitutions may properly be. The function of the Federal Constitution is primarily that of drawing a line between national and state powers, and such a line should not be subject to frequent variations." [37]

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[37] Dodd, Opus cit., 354.

THE ARRANGEMENT AND ACTION OF MATERIAL IN THE PLASMATIC LAYERS AND CELL-WALLS OF PLANTS.

By DR. D. T. MacDOUGAL.

(*Read April 25, 1924.*)

The purpose of the present paper is to present some collated information on the arrangement of colloidal material in the plasmatic layers and walls of the plant cell which are to be taken in account in measurements of permeability, and to present some results of the action of constructed, dead, and living cells in absorption and endosmosis.

Permeability is used in this and previous publications to denote the condition of the colloidal meshwork of the wall and plasmatic layers, which may vary in active cells so that only water and the more mobile ions may pass through when permeability is least and the largest molecular particles go through when it is greatest.[1]

The composition and arrangement of materials in the plasmatic layer and the cell-wall has been discussed in previous papers but it is necessary for the sake of clearness to recapitulate the principal facts and to illustrate the matter with Fig. 1. The wall has a semirigid skeleton of cellulose fibers which are capable of but slight changes of volume or of deformation with water content. The spaces in the external part of this meshwork are occupied by pectins or pectates and the inner part of the meshwork nearest the plasmatic mass contains other liquefiable pentosans, such as mannosans and glucosans, while some lipins or phosphatides occur.

The plasmatic mass is made up of amino-acid chains, pentosans and phosphatides, lipo-proteins, glycolipins, etc., the concentration in the bounding layer of the protoplasm being denoted by the closer spacing of the symbols used for the main components.

[1] MacDougal, D. T., "Permeability and the Increase in Volume of Living and of Artificial Cells," *Proc. Amer. Phil. Soc.*, 72, 1, 1923.

One of the first and most important features of the structure described is its incessant variation in composition. The wall which is at first largely composed of liquefiable pentosans progressively includes a larger proportion of the anhydrides as the wall becomes denser and more rigid. Later pectin compounds appear and the maturity of the wall brings it to a condition in which almost any of the salts or organic compounds of the cell as well as fat-soluble substances may pass through the coarser and fixed meshwork. The outer part may undergo secondary changes by which it becomes water proof in total reversal of the effect of the previous alterations.

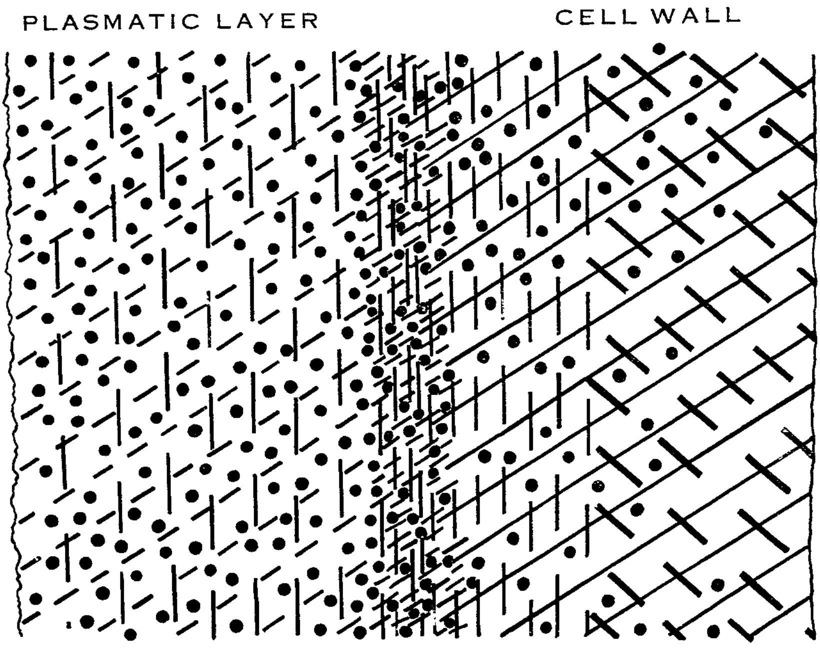

FIG. 1. Diagram of the arrangement of material in the plasmatic layer and cell-wall. ⁄ Cellulose. ╲ Pectin. | Mucilages. ⁄ Proteins. ● Fatty substances.

The plasmatic mass at first high in proteinaceous material and in fatty substances progressively acquires a larger proportion of pentosans or mucilages, with inevitable alterations of reaction to substances included in the vacuole and entering the cell from the medium.

In addition to these changes which come rapidly in short-lived absorbing cells or root-hairs, this complex membrane is highly unstable as to its composition. The displacements which may occur as a result of the various types of adsorption may result in the loss

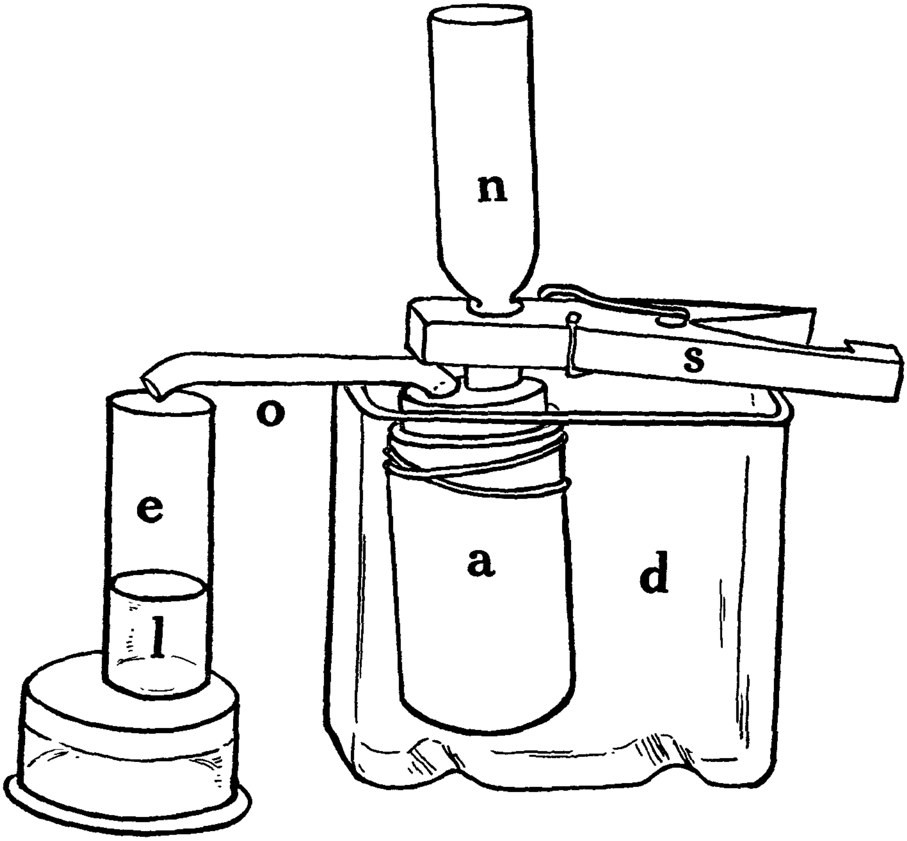

Fig. 2. Colloidal cell in operation. (*a*) Capsule infiltrated and coated with material as in Fig. 1; (*d*) immersion liquid; (*n*) filling funnel; (*o*) outlet; (*e*) receiver; (*l*) liquid delivered as result of endosmose.

of various bases such as potassium and sodium, or the leakage of carbohydrates and amino compounds from the liquid or meshwork of the cell, as has been described by True and his colleagues.

In view of these facts it is obvious that the permeability phenomena observed in such strict and durable membranes as those of

collodion and of parchment subtend but a narrow angle in any comprehensive view of the operations which attend the passage of electrolytes for example through the enormously complex and continuously variable permeable layers of the cell. Referring again to the diagram (Fig. 1), it is to be noted that this is illustrative of a cell which presents a free surface to the medium. The union of two cells presents an arrangement in which this is doubled, the pectinized layer forming the middle part of the wall and the passage of an ion from one cell to another is attended by all of the adsorptive action of two plasmatic layers, which would never be exactly identical in the two protoplasts.

The model of a "constructed cell" used in these experiments consisting of a cellulose extraction thimble infiltrated and coated with materials represented in the living cell differed from previous description only in certain mechanical features. Its setting is illustrated by Fig. 2. By the omission or addition of substances an analysis may be made as to the parts played by the separate components of the permeable layers. The action of the cell as thus modified may be taken in terms of endosmose, measurements of resistance of immersion liquids or cell contents, or by titrations.

The following points are to be noted in the interpretation of the results which follow: first, that as the permeability of the cell lessens the rate of endosmose with any given concentration of sugar as cell-contents will rise within the limits of these experiments; second, that the relative action of the common bases on the meshwork of colloids in modifying permeability rests upon the total action of these bases on the included substances: this action may include several types of adsorption, and third, that the cell-contents generally of 20 per cent. solution of cane sugar had an osmotic potential of about 15 atmospheres, common in plant cells, while that of the external or immersion liquids was much less than 1 atmosphere.

Confirmation of the principal conclusions as to the relative action of similar neutral salts of the common bases has been made. Such action runs generally parallel to the ionic mobilities of the bases which is a straight line function of the charges they carry. Other factors, the effects of which are still being measured, are doubtless to be taken into account. The relative action of these bases is modified by the

common ions in an order determined by the balance found between the charge on the anion and that on the kation. Thus the common anions $SO_4 < Cl < NO_3$ exert a retarding effect, lessening the action of the kations on colloids in an increasing series as given. This action is in conformity with the hydration effects on agar as measured in 1921, as repeated in Table I.; and may be taken to apply within a certain range of effects in which swelling in various degrees results from hydration. Abrupt alterations of effect ensue when the concentrations rise to a point where total neutralization and aggregation of the colloid is brought about.[2]

TABLE I.

HYDRATION OF AGAR IN SULPHATES AND NITRATES.
(Increases in percentage of volume.)

	Na.		K.	
	0.01*M.*	0.0001*M.*	0.01*M.*	0.0001*M.*
Sulphates	740	2215	710	2515
Nitrates	990	2720	827	2910
Water	2400		2560	

The retarding effects of anions on common bases in permeability effects with young roots were found by Kahho to form the following series.[3]

Citrate < Sulphate < Tartrate < Cl < NO_3 < Br < I.

Raber obtained the series below with salts of sodium,[4]

SCN < I < Br < NO_3 < CI < Acetate < Sulphate
< Tartrate < Phosphate < Citrate.

In my own experiments similar permeability effects were obtained with sodium salts in complete cells and with others from

[2] See Freundlich, H., "Capillarchemie," 2te Aufl., pp. 572 ff., 1922. MacDougal, D. T., "The Action of Bases and Salts on Biocolloids and Cell-masses," *Proc. Amer. Phil. Soc.*, 70, 15, 1921.

[3] Kahho, H., "Ein Beitrag zur Permeabilitat des Pflanzenplasmas für Neutralsalze," *Biochem. Ztschrft.*, 123, 284, 1921.

[4] Raber, O., "Permeability of the Cell to Electrolytes," *Botan. Gazette*, 75, 298, 1923.

which the lecithin had been omitted, the series being

Nitrate < Chloride < Sulphate.

Potassium salts gave the series

Iodide < Nitrate < Chloride < Sulphate.

The most useful explanation which has as yet been offered in this matter is that of Raber in which these effects are taken to rest upon the relative density of the charges upon the kations and ions acting upon the colloids. This is in fact implied in the general interpretation put out by Kahho two years earlier. The entire matter of interferences, or antagonisms, may yet be found to lie within the operation of such effects coupled with the special reactions of the albuminous material to sodium and of the pentosans and phosphatides to calcium.

Influence of the H–OH Concentration on Hydration and Permeability.

Results of hydration tests of agar obtained in 1921 show that agar exhibits a greater swelling in dilute solutions of neutral salts than in water. It was also found that this pentosan shows two maxima with respect to the H–OH concentration. One is in the region of HCl 0.0001N about pH 4.2 and the other is in hydroxides at about pH 11, as illustrated in the following table:

TABLE II.

Hydration of Agar in Chlorides.

(Increases in percentage of volume.)

Hcl	KCl	NaCl	$MgCl_2$	$CaCl_2$		
700	1305	1145	825	705	at	0.01M
1600	2900	2680	1590	1190	at	0.001M
2100	2645	2975	2315	2160	at	0.0001M
Water	1900					

Hydration of Agar in Hydroxides.

LiOH	NaOH	RbOH	KOH	$Ca(OH)_2$		
1820	1645	1635	1535	860	at	0.01N
3430	3430	3500	3430	1220	at	0.001N
				3200	at	0.0001N
				3200	at	0.00001N
Water				3000		

The hydration swelling of gelatine as representative of the ampholytes is such that increase in concentration of acid or of hydroxide is accompanied by increased hydration and permeability within the biological range of concentrations. It is to be noted, however, that two points of minimum swelling have been found, one at PH 4.7 and at PH 7.7 as determined by Wilson and Kern, and at PH 4.68 and 7.66 as fixed by Higley.[5] The effect of salts within the biological range is to increase swelling of gelatine, but it is to be noted that the swelling of ampholytes such as gelatine as a result of the acidity is lessened or repressed by the presence of salts.

Any consideration of the action of the cell-colloids must proceed upon the accepted fact that hydration and permeability are a resultant of the action of the included constituents. Thus, by way of illustration, the swelling of gelatine was found to be as below by auxographic tests:

TABLE III.

KCl	2100 per cent.	$CaCl_2$	670 per cent. at 0.01M
	1400 per cent.		1596 per cent. at 0.001M
	1160 per cent.		1000 per cent. at 0.0001M
WATER	910 per cent.		

The swelling of mixtures of 3 parts of gelatine with 2 parts of agar in neutral salts gives the following values:

TABLE IV.

KCl	880 per cent.	$CaCl_2$	740 per cent. at 0.01M
	1010 per cent.		940 per cent. at 0.001M
	1850 per cent.		1430 per cent. at 0.0001M
WATER	1420 per cent.		

The mixture consisting of 3 parts agar and 2 parts gelatine which reproduces the plasmatic composition of the higher plants more exactly shows much greater swell capacity, as shown by the figure below:

TABLE V.

KCl	2015 per cent	$CaCl_2$	1140 per cent. at 0.01M
	2270 per cent.		1220 per cent. at 0.001M
	2640 per cent.		2268 per cent. at 0.0001M
WATER	2330 per cent.		

[5] Wilson, J. A., "The Swelling of Protein Jellies," Colloid symposium monograph, Dept. of Chem., Univ. of Wisconsin, 1923, p. 210.

The degree of hydration or coefficient of swelling of cell-colloids is also the index of permeability, which is invariably a resultant of the reactions of the pentosans and protein gels. Materials of these two groups differ markedly, as one is an ampholyte dissociating both H and OH ions, showing two isoelectric zones in which there is no movement of H ions, and the pentosans generally dissociate some H ions, being weak acids.

The resultant action when a composite mass of the two substances is acted upon by acids or alkalies or by neutral salts will be determined by the proportions of the two present and the relation of the ions in the hydrating liquid to the neutral points, isoelectric zones or other conditions of the two groups of colloids. The permeability of a protein jelly will be least in the isoelectric zones as the gels of the protoplasm when in this condition have a lessened permeability and the cell will show the highest endosmotic swelling or turgidity. As noted above, increased H or OH concentration in the hydration solution or increased concentration of an electrolyte increases hydration and permeability in proteins, while it lessens permeability and increases possible endosmotic action in pentosan gels or mucilages. The solutions which cause a minimum hydration and permeability and the greatest endosmotic swelling may not therefore be the "isoelectric points" of a vacuolated cell, as suggested by Robbins,[6] but a resultant, or zone in which the low hydration value of a protein is met by a decreasing hydration value of the pentosan. The greatest swelling of a dense non-vacuolar cell would be a similar zone of conjunction of the highest hydration possibilities.

Zones of Maximum and Minimum Endosmose and Permeability in Constructed Cells.

The determination of the zones of concentration in which cells would show maximum and minimum effects was tested with constructed cells as well as with sections of *Opuntia*.

The initial experiments in such a test were made with colloidal cells constructed in sets in which one or more of the essential components were omitted. The action of complete cells was compared with that of living and dead cells.

[6] Robbins, W. J., "An Isoelectric Point for Plant Tissue and Its Significance," *Amer. Jour. of Bot.*, 10, 412, 1923.

One set of cells with contents of 20 per cent. sugar solution and with the layers composed wholly of agar and pectin gave the following amounts of endosmose:

TABLE VI.

Immersion.		Amount of Endosmose in 48 Hours.
HCl	0.01N	14 cc.
	0.001	12
WATER		12
NaOH	0.001	9
	0.01	17
NaCl	0.01	
HCl	0.001	7.7
NaCl	0.01	
HCl	0.01N	9.5

The characteristic pentosan reactions are illustrated. As the acidity of the immersion or hydration solution increased permeability lessened and the cell thus showed greater endosmose or turgidity at HCl 0.01N than at 0.001N. A similar relation was found on the other side of the neutral point, the turgidity or endosmose being greatest as the OH concentration increased and the swelling lessened.

Acidification of a salt showed similar effects, the increase of acidity from 0.001 to 0.01N lessening permeability and increasing turgidity. The reverse effects to be expected from protein gels are too well known to need repetition.

The next series of tests were made with cells constructed as in the previous set with the addition of gelatine to the plasmatic layer. The results were as follows:

TABLE VII.

Immersion.		Amounts of Endosmose in 48 Hours at 16–18° C.
NaOH	0.005N	12 cc.
	0.0025	22
	0.001	15
Water		21
HCl	0.001	12
	0.0025	10
	0.005	15

NaCl	0 01*M*	
NaOH	0.001	8
NaCl	0.01*M*	
NaOH	0 0025	9
NaCl	0.01*M*	
NaOH	0.005	15
NaCl	0.01*M*	10
NaCl	0.01*M*	
HCl	0 001*N*	9
NaCl	0.01*M*	
HCl	0 0025	13
NaCl	0 01*M*	
HCl	.005	26

The results in Table VII. show maxima which are resultants of the swelling coefficients of the pentosans and amino-acid gels. The maximum turgidity and endosmose coincident with minimum hydration swelling of the two components was in 0.0025 of the acid. A corresponding node was found in the hydroxide at 0.005*N*.

The addition of a salt carried progressive decrease of permeability with the increased H or OH concentration.

TABLE VIII.

Immersion.		Amount of Endosmose.
HCl	0 001*N*	11 4 cc.
	0.005	11.5
Water		13.8
NaOH	0.001	7 2
	0.005	13 8
NaCl	0.01*M*	
HCl	0.001*N*	11
NaCl	0.01	
HCl	0.0025	10.4
NaCl	0 01*M*	
HCl	0.001	11
NaCl	0.01*M*	6.5
NaCl	0.01	
NaOH	0.001*N*	9.8
NaCl	0 01	
NaOH	0.0025	10 2
NaCl	0.01*M*	
NaOH	0.005	8

A third series of tests were made with cells to which a lecithin deposit had been made in the plasmatic layer, the cell including agar,

pectin, gelatine and lecithin, the gelatine forming about two fifths of the plasmatic layer. The results of the endosmotic action of such a series of cells with contents of 20 per cent. sugar solution operating for 42 hours are given in Table VIII.

The presence of the lecithin in these cells may be held to account for an equalizing action or a broadening of the zones of maximum or minimum. Thus no variation was seen in the permeability in 0.001–0.005 water-acid. Neither did the acidification of the salt show much effect, a flattening which is to be seen in the results with living and dead cells. Permeability lessened in water-hydroxide solution as the concentration was increased to 0.005 and the maximum lay beyond. The addition of hydroxide to the salt showed a node at NaOH 0.0025.

Another series of complete cells were placed in water-acid solutions with the result that a node was found in concentrations of 0.001–0.0004*N*, at which endosmose was the highest, below and above which it fell off. The composition of the cells differed from those in Table VIII. chiefly in a higher proportion of agar in the plasmatic layer.

Distention of Living Cells of *Opuntia* in Acids, Hydroxides and Neutral Salt Solutions.

The flattened joints of *Opuntia* offer exceptional advantages for the analysis of permeability and distention measurements as something is known as to the plasmatic mass especially as to its high pentosan content, and the daily variation of the acidity of the cell sap from near neutrality to a concentration which may be as great as pH 2.5 or even higher (nearly equivalent HCl 0.01) has been worked out with exactness and in great detail by Richards.[7] It was found that the titration values of the acidity might vary as 4 to 1 or even as much as 7 to 1 at certain times.

Material taken before 8 A.M. would have a sap near the maximum and that taken at 3 P.M. would be as nearly depleted of acid as may be found at any time.

Thin slices taken from the median layer of the joints were placed in Stender dishes in trios, and their variations in thickness measured

[7] Richards, H. M., "Acidity and Gas Interchange in *Cacti*," Publ. 209, Carnegie Inst. of Wash., 1915.

and recorded with the auxograph of the model used extensively in this laboratory. Comparisons were made only in sets of equivalent material taken at the same time and hydrated in the same range of temperature which was usually at 14–18° C.

The first group of tests was made on three sets of sections taken in the morning and three in the afternoon and hydration was made in water and water-acid and hydroxide solutions.

The three sets of sections in the acid condition showed the greatest distention or turgidity resulting from the least permeability in HCl 0.0025, equivalent to about pH 2.5, which would be less acid than the cell-sap. Sections in the acid-depleted condition showed the greatest distention or turgidity and hence the least permeability in HCl 0.001, about pH 3, which was probably more acid than the cell-sap. Effects were lessened at higher and lower concentration in both cases.

Distention and turgidity and a minimum permeability was found on the OH side of neutrality in NaOH 0.0025 in the morning or more acid condition of the cells as well as in the acid-depleted condition in the afternoon. This would be equivalent to about pH 12.

All distentions were greater in the acid-depleted condition of the afternoon than in the morning although the dendrographic records show that the plant has a higher water content at this hour than in the morning.

TABLE IX.

Solutions.		Increases. 8.30 A.M.	3.00 P.M.
NaCl	0.01*M*	8 p.ct.	16 p.ct.
	0.005	10	16
	0.0025	8.8	16
Water		10	21
NaCl	0.01		
NaOH	0.001*N*	15	17.5
NaCl	0.01		
NaOH	0.0025	15	20
NaCl	0.005		
NaOH	0.005	13	22
NaCl	0.005		
HCl	0.005	8.5	10
NaCl	0.005		
HCl	0.0025	9	15
NaCl	0.01		
HCl	0.001	11	18

Another series comprised sections taken in the morning and afternoon and hydrated in salt solutions made acid and alkaline as shown in Table IX.

The maximum distention in the neutral salt solution was found in the highly acid material of the morning at a concentration of $0.005M$. No variation was found in the concentration of $0.01M$ -0.0025 in the acid-depleted material of the afternoon.

The maximum distention and turgidity and hence least permeability of the living sections in the acid condition as well as in the acid-depleted condition was in NaCl $0.01M$ + HCl $0.001N$. The distention of the highly acid material of the morning in salts made alkaline was greatest in NaCl $0.01M$ + NaOH $0.001N$ and NaOH 0.0025, the nodal zone evidently being a broad one. The acid-depleted material showed the greatest distention in NaCl $0.01M$ + NaOH $0.005N$. This lot of material differed from that described above in the reaction to alkali, a variation which might reasonably be attributed to the condition of the material.

Distention and Swelling of Dried Slices of *Opuntia*.

When median slices such as those which are used in a living condition in the previous tests are dried under a pressure to keep them plane for three or four days, the result is sections about one fourth the original thickness, of a leathery consistency, in which the turgidity of the cells has been lost, the walls have collapsed and permeability conditions of the whole alter in several ways. It seems clear, however, that the hydration of such sections is attended by some osmotic action as well as the swelling resulting from the hydration of the desiccated colloids.

Hydration of a great number of sections in a series of water-acid solutions showed the greatest increase at $0.01N = \text{pH } 2$, which may be taken as a node of the swelling action of the pentosans and protein gels present, the absolute maxima lying beyond the range of the solutions. In this case the action of the acid would be to accentuate the swelling of the amino compounds and to lessen the permeability of the pentosans with rising concentration. On the other side of neutrality the increase is greatest in the hydroxide at $0.001N = \text{pH } 11$ in confirmation of previous results.[8]

[8] MacDougal, D. T., "Permeability and the Increase in Volume of the Contents of Living and Artificial Cells," *Proc. Amer. Phil. Soc.*, *72*, 1, 1923;

The maxima were in more acid and less alkaline solutions than those producing the greatest distention in living cells.

Dried sections in solutions of NaCl made acid by the addition of HCl showed maximum increase in NaCl 0.01 + HCl 0.001*N* and in similar salt solution made alkaline by the addition of NaOH 0.001*N*, in one set; in another the maximum in the acid was at the same concentration but that in NaOH was at 0.005*N*.

The zone of greatest increase on the acid side would lie in the region of pH 3 while that on the OH side of neutrality would lie in the region of pH 11–12.5.

Recounting the facts concerning the nodal points described on the preceding pages, it is to be stated that agar plates show the highest hydration values at pH 4.2 and pH 11. Between these two swelling falls to that of water and beyond these points the increase lessens with the rising concentration of acid and alkali within the biological range. Protein gels show increased swelling in H and OH concentrations away from the isoelectric zones and in increasing concentration of neutral salts. The swelling of mixtures of pentosans and amino compounds is a resultant of the reactions implied.

Permeability rises with the degree of hydration.

Colloidal cells of agar and pectin showed a permeability minimum at pH 3 equivalent to that in water with a maximum beyond pH 2. Another maximum was found beyond in the region of pH 11. Similar relations were found in acidified salts and in those made alkaline with NaOH.

Cells constructed of agar-pectin and gelatine showed maximum permeability with a minimum endosmose at pH 2.5–3 and pH 11–12. Salts solutions made acid and alkaline showed permeability decreasing away from neutrality in water more acid than pH 3 and more alkaline than pH 11.

The inclusion of lecithin in the construction of the cell resulted in a displacement of the points of permeability, especially with respect to the effect of acid. One set of cells showed but little change in permeability between 0.001*N* and 0.005*N* HCl but in another series the minimum of permeability in the concentrations of HCl 0.001–0.004*N*.

"The Probable Action of Lipoids in Growth," *Proc. Amer. Phil. Soc.*, 71, 33, 1922.

A minimum of permeability in hydroxide was found at NaOH 0.0025, more dilute and more concentrated solutions giving less endosmose.

If now these conceptions be applied to the dead cell-masses, the greatest increase of such material was in solutions more acid than pH 2 and between pH 11 and 12.

Living cell-masses showed the greatest distention at about pH 2.5 to 3 and in the region of pH 12 and above.

It is to be noted that the maximum increase in a nonvacuolar mass would be one of hydration in which permeability would be progressively lessened, a procedure which would be characteristic of the enlargement of cells in the earlier or accretion stage of growth.

The maximum increase of a vacuolated cell on the other hand would be coincident with minimum permeability in which the cell contents would exert the greatest osmotic action.

In the case of the dead cell-masses the progressive rise of the swelling away from the neutral point might be due to an action similar to that described for protein gels; on the other hand it is to be noted that the cells of *Opuntia* are high in pentosans and that the permeability of such material would be progressively lessened with increased H or OH concentration with the result that osmotic distention of the cells would follow.

INFLUENCE OF CONCENTRATION OF CELL-CONTENTS ON ADSORPTION OF IONS FROM THE MEDIUM OR IMMERSION LIQUID.

A series of tests was arranged in which the relation of the concentration of the cell-sap to the external solution to the adsorption of the common kations was determined.

In one arrangement cells with contents of 20 per cent. sugar solution with others containing 0.6 per cent. sugar solution were placed in chlorides of potassium, sodium and calcium at 0.01*M*. The higher concentration would have an osmotic potential of about 15 atmospheres, while the weaker would be equivalent or hypotonic to the salt solutions. It is to be noted that some plants of arid regions such as the *Atriplices* may have tissue fluids with a potential of over 150 atmospheres.

After the cells were operated for periods of 40 to 48 hours the immersion liquids were titrated for chlorine which would show how

much of the anion had been adsorbed by the cell. It was then assumed that the kation had been adsorbed in equivalent measure, which would not be exactly correct; the difference, however, would not constitute a serious error in the magnitudes obtained.

Chlorine determinations were made by direct titration of 25 cc. of the liquid with $AgNO_3$ 0.01*M*.[9] Since deviations in the concentration of the salt solutions played small part in the adsorption no great care was exercised in making them up, but the chlorine was titrated in the originals and then at the close of the experiment. The results are expressed in cc. of the silver nitrate necessary to effect the titration. Table X. gives results from agar-pectin-gelatine-lecithin cells; 3 parts of gelatine to 5 parts of agar were used in the plasmatic layer.[10]

TABLE X.

Contents, Sugar Sol.	Immersion.	Endosmose.	$AgNO_3$.	
			Original.	Final.
20 per cent...	NaCl 0.01*M*	9.6	29	26.2
0.6 per cent..	"	3.5	"	27.9
20 per cent....	KCl 0.01*M*	8	39.7	30
0.6 per cent.	"	1.4	"	27.2

A set of agar-gelatine-pectin cells with lecithin omitted with contents of 20 per cent. sugar solution gave the following:

TABLE XI.

Immersion.	Endosmose.	$AgNO_3$.		Percentage of Absorption.
		Original	Final.	
NaCl 0.01*M*.. ..	6	24.2	20.9	13.3
0.0025 . ..	6.5	13.5	12.5	7.5
KCl 0.01*M*.......	7	24.55	20.9	15
0.0025......	7.1	13.6	12.2	10
$CaCl_2$ 0.001*M*.	9	50.5	46.3	8.3
0.0025	7	15.8	10.6	33 (?)

[9] I am indebted to Professor J. W. E. Glattfeld, of the University of Chicago, for the titrations.

[10] See Gortner, R. A., and Hoffman, W. F., "Quantitative Estimation of Chlorides and Sulphates in Expressed Plant Tissue Fluids," *Botan. Gazette*, 77, 96, 1924, for additional methods.

The rate of endosmose formed the series Na < K < Ca in both concentrations of the immersion solution. The rates of absorption of chlorine from the solutions of the higher concentration formed the series K > Na > and > Ca. The rate from the weaker solution formed the series Ca > Na > and > K, a reversal of the action of Ca and K which may be due to some unknown features of the cell.

Another set gave the following data (agar-gelatine-pectin-lecithin cells):

TABLE XII.

Cell Contents Sugar Sol.	Immersion.	Endosmose.	$AgNO_3$. Original.	$AgNO_3$. Final.	Percentage of Absorption.
20 per cent . .	NaCl 0.01*M*	10	24.5	21	14.3
0.5 per cent.	"	0.0	"	21 3	13
20 per cent. . .	KCl 0 01*M*	12.3	24.9	20.7	17
0.5 per cent. .	"	0.0	"	21.5	13
20 per cent. .	$CaCl_2$ 0.01*M*	16.2	16 9	14 9	11.8
0 5 per cent. . . .	"	0.0	"	21.5	11.5
20 per cent.	Seawater 0 01*M* for Na	11.2	28 9	25 3	12 8
0 5 per cent .		.2	"	25.2	12 8
20 per cent. . .	(NaCl 0 005) (NaOH 0 005)	13.4	(50 cc.) 46.7	28.5	39.7
0.5 per cent. . .	"	0.0	"	28 8	38.4
20 per cent. . .	(NaCl 0.005) (NaOH 0.005)	11.8	(50 cc) 46 9	41 1	14.1
0 5 per cent. . .	"	0.0	"	40.6	15.3

The permeability series of these cells was NaCl > Seawater > KCl > $CaCl_2$. The amounts of chlorine adsorbed formed the series KCl > NaCl > Seawater > $CaCl_2$. The adsorption of chlorine was seen to be very much greater in the alkaline solution than from the acid-salt solution. The influence of the concentration of the cell-contents on absorption was noticeable in potassium only, which was probably due to faulty operation, or some defect in the cells. The average absorption of chlorine by the cells with 20 per cent. sugar solution was 18.3 while it was 17.3 for the cells containing only 0.5 per cent. sugar solution, a difference largely due to the discordant action of the cells in the potassium solution. A similar discrepancy in the action of cells was found in the sodium solution in the following series.

In this series the adsorption of the salt was much greater from alkaline than from neutral solutions, and the adsorption of K much

greater than that of Na. It is also seen that the adsorption of ions by the colloidal cell was but little influenced by the concentration of the cell-contents; the average loss of chlorine from all immersions of cells with concentrated contents was 35.3 per cent.; to cells with dilute contents it was 34.5 per cent.

TABLE XIII.

Contents	Immersion.	Endosmose.	$AgNO_3$		Percentage of Absorption.
			Original.	Final.	
20 per cent.	NaCl 0.01*M*	10	23.5	18	23
0.6 per cent.	"	0.0		10.8	28.5
20 per cent..	(NaCl 0.01) (NaOH 0.001)	11.6	22.8	16.4	28
0.6 per cent. .	"	0.0	"	16.5	27.6
20 per cent..	(NaCl 0.01) (NaOH 0.005)	8	27.5	16.7	40
0.5 per cent..	"	0.0	"	18	35
20 per cent..	KCl 0.01	11.2	24.1	18.6	22.8
0.5 per cent.	"	0.0	"	18.2	24.4
20 per cent	(KCl 0.01) (KOH 0.001)	9.6	27.2	14.1	48
0.5 per cent. .	"	0.0	"	15.9	41.5
20 per cent.	(KCl 0.01) (KOH 0.005)	10	30.5	14.9	50
0.5 per cent	"	0.0	"	15	50

While the current conceptions of the physical action of the cell make it obvious that the passage of electrolytes into the cell-material is affected very little by the osmotic potential of the cell-contents, this conclusion has not been directly or adequately tested. The foregoing tests show that the adsorption of chlorides by constructed cells with dilute contents was but slightly different in amount from the adsorption by similar cells with concentrated contents. The average adsorption of chlorides from neutral and alkaline salts in one series by cells with a 20 per cent. sap was 18.3 per cent. of the immersion, while 17.3 per cent. was taken up by cells with contents of 0.6 per cent. sugar solution. In another series cells with the more concentrated sap took up 35.3 per cent. of the chlorides while those with the dilute contents adsorbed 34.5 per cent.

The relative amounts of the various salts which may be taken up vary with the composition of the cell and other controllable factors. The well-known acceleration of adsorption in solutions made alkaline is strikingly exemplified.

DEPT. BOTANICAL RESEARCH,
CARNEGIE INSTITUTION.

THE FAUNAS OF THE CONCRETIONARY ZONES OF THE OREODON BEDS, WHITE RIVER OLIGOCENE.[1]

By WILLIAM J. SINCLAIR.

(*Read April 25, 1924.*)

CONTENTS.

I. INTRODUCTION AND SUMMARY.

When the Princeton 1920 Expedition to South Dakota was being planned, I hoped that we might succeed in locating one or more zones or levels in the White River beds, of wide areal extent and rich fossil possibilities, which would afford large assemblages of con-

[1] Investigation aided by a grant from the Marsh Fund of the National Academy of Sciences.

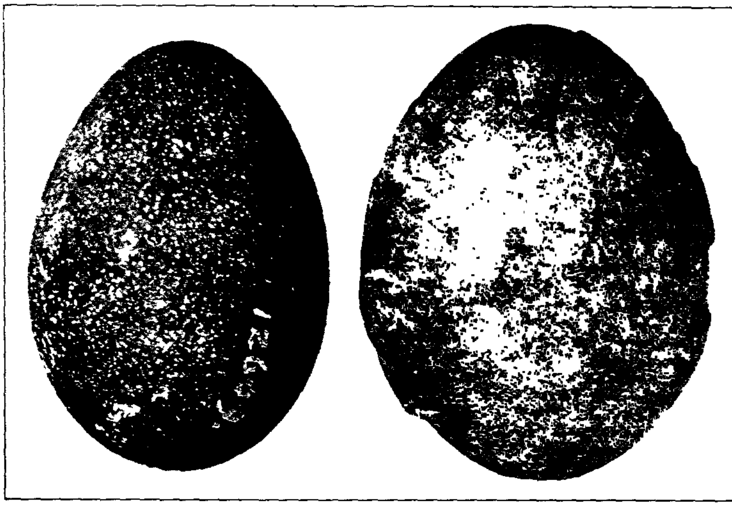

FIG. 1. Fossilized eggs of birds, Nos. 12609 and 12616 Princeton University Geological Museum. Natural size.

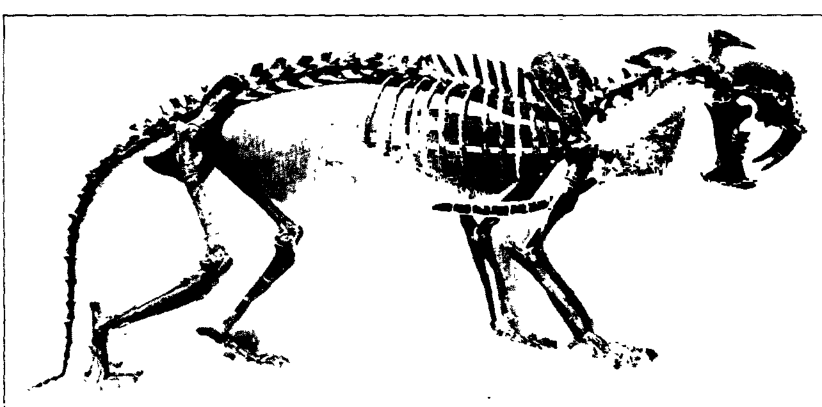

FIG. 2. *Hoplophoneus primaevus* Leidy. Mounted skeleton, No. 12750 Princeton University Geological Museum. Height at shoulder 14 inches.

temporary forms and so act as controls in future stratigraphic and evolutionary studies. From Dr. Wortman's[2] brief notes on his section, published in 1893, the idea occurred to me, long before I had seen more than its characteristic fossiliferous nodules, preserved in the Princeton and American Museum collections, that his "red layer" in the Lower Oreodon beds would prove to be a climatically controlled feature, owing its concretions to the evaporation, at or near the surface of the ground, of sub-surface waters, rising by capillarity under conditions of aridity, and would, accordingly, be easily recognizable in widely separated parts of the badlands, would coincide with an old land surface or series of surfaces on which deposits formed during the arid episodes, and, affording an abundance of well-preserved fossils, as it promised to do, would be an ideal control horizon.

It was also planned to accumulate data for a new section, as that in general use(Dr. Wortman's) was manifestly in need of revision, being based on field methods of an earlier date. The origin of the White River sediments also called for investigation. These had hitherto been described in general terms, such as fuller's earth, sands, clays, ash, etc. By determining their nature and tracing the sedimentary materials to their sources there might be something to learn about life conditions at the time this accumulation took place. From the nature of the contacts separating the various elements of the White River series, and these in turn from the Pierre shale below, it was hoped to form some conception of the ancient physiography and so ascertain the kinds of land surface over which deposition occurred, as these mark the longest halts in the sedimentation process. In these investigations I have been associated with Mr. H. R. Wanless, who has undertaken the study of the lithology and stratigraphy of the White River series in parts of Pennington, Washington and Jackson counties, South Dakota, and has presented his results in two able papers already published,[3] wherein many of the problems outlined above are discussed.

[2] Wortman, J. L., "On the Divisions of the White River or the Lower Miocene of Dakota," *Bull. Am. Mus. Nat. Hist.*, Vol. V., 1893, pp. 95–105.

[3] Wanless, H. R., "Lithology of the White River Sediments," *Proc. Am. Phil. Soc.*, Vol. LXI., pp. 184–203, 1922; "The Stratigraphy of the White River Beds of South Dakota," *Proc. Am. Phil. Soc.*, Vol. LXII., pp. 190–269, 1923.

The collecting season of 1920 demonstrated the amazing fossil richness of the "red layer" in what was, supposedly, well-worked territory, and we were fully occupied in following the narrow outcrop of its noduliferous portion in the basins of Bear, Spring, and Indian Creeks in Pennington County and at Interior in Jackson County. Its wide areal extent was realized, while its limited vertical thickness demonstrated that it was indeed an ideal control horizon. Frequent excursions into the Titanotherium beds below gave little encouragement to the expectation of finding there stratigraphic zones which could be traced more than a few hundred yards, although in some places fossils were in considerable abundance.

The observations of the first season were presented in my paper entitled "The Turtle-Oreodon Layer" or "Red Layer," a Contribution to the Stratigraphy of the White River Oligocene (Results of the Princeton University 1920 Expedition to South Dakota)[4] in which the "red layer" as then known to us was described and the hypothesis of climatic control formulated.

In subsequent summers (1921 and 1922) with the aid of grants from the Marsh Fund of the National Academy of Sciences, the "red layer" was followed southwestward and westward and our collections, stratigraphic data, and conception of the "red layer" itself greatly expanded. Search for higher datum planes resulted in the re-location of Wortman's upper noduliferous zone of the Oreodon beds some 60 feet above the top of the "red layer."

Fuller acquaintance with these two noduliferous zones brought out, as discussed in full in Mr. Wanless' papers, that the upper one was extremely thin and of limited areal extent, while the lower is a well-marked stratigraphic feature, traceable for miles, always abundantly fossiliferous, constituting an acceptable mapable unit, in fact, without exaggeration, the most remarkable fossil level in any of our North American Tertiaries.

When seen by us in 1920, the concretionary level, constituting Wortman's "red layer," was supposed to be confined to the upper part of the 40-odd feet of pinkish-gray clays constituting the lower Oreodon beds, but many observations made since then have demonstrated that additional sheets of nodules may be introduced below,

[4] *Proc. Am. Phil. Soc.*, Vol. LX., No. 3, pp. 457–466, 1921.

which increase, not only its thickness, but the difficulty of defining its lower limits, so that it has been found expedient to designate the whole zone of pinkish-gray clays at the base of the Oreodon beds as the "Turtle-Oreodon layer" or "red layer," no faunal change having been noted throughout it. Reference to the lower zone of rusty nodules, or lower concretionary zone, in what follows, applies to the concretionary phase of this pinkish-gray clay, which sometimes entirely lacks nodules.

It must not be assumed that fossils are absent from the clays of the Oreodon beds outside of these zones, for such is not the case. The upper Oreodon clays in the vicinity of Interior afford fragmentary vertebrate remains in considerable amount. Occasional specimens may also occur in areas far apart from each other and difficult to bring into zonal relationship.

Previous to our work, the major faunal phenomena of the White River Oligocene had been well established, and still remain so. Our collecting experience supports Dr. Matthew's [5] recognition of two faunal facies, characteristic of the Oreodon clays and Metamynodon sandstone channels respectively, representing local differences in habitat. As shown in Mr. Wanless' stratigraphic report, the Metamynodon sandstones were deposited in stream channels occurring at various levels all below the upper concretionary zone, and certain elements in their fauna, such as *Metamynodon* and the anthracotheres, found rarely outside these coarse channel deposits, but commonly within them, may be presumed to have been of aquatic habit. The analogy of the "clays" fauna with that of the modern plains has been commented on by Dr. Matthew,[6] and is supported by Mr. Wanless' lithologic studies. These indicate sheet-flood action of temporary duration, over a wide plain traversed by rivers (the Metamynodon channels) with extremely low inter-stream divides, far lower than exist today in the present high plains region with its intrenched drainage. Intervals of aridity resulted in the formation of the concretionary zones as already indicated, so that on the basis of both lithologic and stratigraphic evidence, presented in Mr. Wan-

[5] Matthew, W. D., *Memoirs of the American Museum of Natural History*, Vol. I., Part 7, p. 368 et seq.

[6] Loc. cit., p. 371.

less' papers, their faunas are regarded. not only as a plains assemblage, but one characteristic of, at times, arid plains, where arboreal vegetation, represented only by the innumerable hackberry seeds occurring everywhere in the lower concretionary zone, was possibly limited to the margins of the more or less permanently water-bearing channels, much as are the cottonwoods, hackberries, etc., in the bottoms of the modern draws. The occasionl flooding of these would account for the hackberry seeds in the inter-stream clays, and also for the rapid sweeping together of fragments of the dry surface clays, unprotected by vegetation, to form the clay-pellet conglomerates of the concretionary zones.

No fauna of Oligocene age which could definitely be associated with an old land surface, or series of closely contemporaneous land surfaces, has ever been identified, and the recognition of such a fauna constitutes one of the more important of our results. The second point which may be emphasized is the discovery, within many groups, of numerous contemporary variations. Some of these have already been published,[7] and others will be discussed in the pages which follow; some are interpretable as individual variations while others seem to be progressive mutations in the deVriesian sense. To distinguish between them, where but few specimens are available, is difficult. I have endeavored to put them on record without, I trust, adding unduly to the multiplicity of names, and giving, I believe, acceptable reasons for whatever has been done to the synonymy.

My grants from the National Academy were for the study of the "*Mesohippus bairdii-Oreodon culbertsoni* Zone." These forms are the most abundant of all the Oreodon beds species, being represented by individuals far in excess of the numbers indicated by the figures in the lists on a later page, which include only the specimens collected, while many more were examinned and left in the field as too poor to be worth the expense of shipment.

Anticipating, to some extent, what will be said later, we have found that *Oreodon culbertsoni culbertsoni* Leidy is extremely abundant from the base of the Oreodon beds to the top of the lower zone

[7] Sinclair, W. U., "The Small Entelodonts of the White River Oligocene," *Proc. Am. Phil. Soc.*, Vol. LXI., pp. 53–64, 1922. "Hyracodons from the Big Badlands of South Dakota," *Proc. Am. Phil. Soc.*, Vol. LXI., No. 1, 1922, pp. 65–79, 1922.

of nodules. One specimen, with auditory bullæ of rugged type, which I am unable to separate from *Oreodon culbertsoni culbertsoni*, was found by us in the uppermost part of the Titanotherium beds about 18 inches below the contact plane, but not a single specimen of this sub-species was found above the level of the noduliferous portion of the "red layer" in which hundreds of individuals occur. On the other hand, the smaller sub-species, *Oreodon culbertsoni periculorum* Cope, occurs in both concretionary zones, as indicated in the lists below.

Turning now to *Mesohippus bairdii*, this has an equally wide range, occurring in great abundance in the lower nodular zone and in more limited numbers in the upper level, where there are small horses inseparable, so far as I can see, from typical *bairdii*. The Princeton collections do not afford data regarding its possible downward range below the level of the Oreodon beds, but teeth and jaw fragments of *Mesohippus* in the American Museum Collection from the Titanotherium beds, kindly loaned by Dr. W. D. Matthew, seem to me inseparable from *bairdii* and, if correctly identified, indicate a situation similar to the case of *O. culbertsoni culbertsoni*.

If there is need of a faunal designation, then the "*Mesohippus bairdii-Oreodon culbertsoni* Zone," with the limitations imposed by the stratigraphic range of both of its most characteristic species, may be regarded as applicable to the "red layer" as here re-defined.

The discussion which follows is limited to the faunas of these two concretionary zones. Contemporaneity is strictly true of the assemblage from the upper level which is, approximately, a foot thick and usually consists of but a single layer of nodules, while that from the lower zone represents the population of successive land surfaces, receiving sediments during a sequence of closely spaced climatic episodes in which aridity predominated, causing the upward migration of groundwaters and the formation of nodule bands as the result of evaporation. So closely spaced were these arid cycles that the fauna may be regarded as a unit, for any attempt to correlate the faunules of the successive nodule sheets constituting the lower noduliferous zone would be futile, many of the finest specimens collected being in nodules which had been displaced by erosion from their original position.

Whether this recurrent aridity has had anything to do with the decrease in numbers of certain elements in the faunas between the lower and upper concretionary zones (*Oreodon culbertsoni culbertsoni*, for example) is problematical, but may be entertained in view of the absolute extermination of titanotheres at the close of Titanotherium beds deposition where, as we have endeavored to show elsewhere, a climatic factor was probably involved.

It has not proved, and possibly never will prove, practicable to trace the upward range in the Oreodon beds of each species listed. The considerable thickness of scantily fossiliferous clays between the Leptauchenia ash and the upper zone of nodules is discouraging to the collector, and it will be years, if ever, before the upward range of each species into or through this barren zone is established. In fact I know of no non-marine North American Tertiary formation in which the vertical range in feet and inches of any of its characteristic species can be stated, although broad limits can be established, just as have been attempted in this paper for the faunas of the Oreodon beds.

In the lists which follow, the number of specimens of each species collected from the two concretionary zones during the summers of 1920–22 is indicated by figures in the parallel columns to give an idea of what may be expected as a result of several seasons' work. They range from the finest of museum material to poor study specimens, although, as the result of critical selection in the field during two summers under my personal supervision, the quality average is high. Some of the gaps in the list from the upper level are probably due rather to the exigencies of preservation and collection than to the absence of the species in the level recorded, for here every specimen found was taken, irrespective of quality.

That the fauna of the lower level constitutes such a large percentage of the total assemblage of species from the Oreodon beds is not surprising, in view of the fact that most of the collecting in South Dakota, in this part of the White River series, has always been from this level, since that 14th of May in 1850 when it was first seen, in the badlands of Bear Creek basin, by T. A. Culbertson who having "ascertained the locality of these petrifactions to be so circum-

scribed . . . hastened to finish collecting in one day."[8] Culbertson's locality lies about four and one half miles northeast of the town of Scenic in Pennington County, in the bluffs and tributary draws along the northeast side of Bear Creek and is directly continuous with exposures in Chamberlain Pass between the basins of Bear Creek and Cain Creek, along the wagon road and railroad east of Scenic. Here the lower concretionary zone of the Oreodon beds is exposed, the upper level developing no nodules in this part of the badlands. The collection which Culbertson made for the Smithsonian Institution came from the lower concretionary zone, which is the one affording the numerous fossil turtles mentioned by him. Nodules are typically developed but are neither as abundant nor as fossiliferous as in the upper part of Bear Creek basin. Our own collections, made from the great "pocket" examined by Culbertson, and from the bluffs directly continuous therewith in Chamberlain Pass, include *Daphænus vetus, Hoplophoneus insolens, Mesohippus bairdii, Oreodon culbertsoni culbertsoni, Oreodon culbertsoni periculorum, Hyracdoon* sp., *Cænopus* sp., *Ictops dakotensis, Stylemys nebrascensis* and the peculiar variant of *Archæotherium mortoni* discussed on a former occasion and to be again mentioned later.

II. THE FAUNAL LIST.

In the list which follows, only those forms are included which were actually found by us during the summers of 1920–22 in the concretionary zones of the Oreodon beds. Omissions indicate merely our failure to find specimens. The figures in parallel columns show the number of individuals secured.

	Lower Nodular Zone.	Upper Nodular Zone.
CARNIVORA CREODONTA:		
Hyænodon crucians Leidy	5	3
Hyænodon cruentus Leidy	6	
Hyænodon horridus Leidy	3	
CARNIVORA FISSIPEDIA:		
Daphænus vetus Leidy and its variants	7	
Daphænus hartshornianus Cope	1	

[8] Culbertson, T. A., "Journal of an Expedition to the Mauvaises Terres and the Upper Missouri in 1850," Smithsonian Institution, Fifth Ann. Rept., 1851, pp. 84–145.

Cynodictis gregarius Cope	3		
Dinictis felina Leidy	7		
Dinictis squalidens Cope	1		
Hoplophoneus primævus Leidy	3		
Hoplophoneus robustus Adams	1		
Hoplophoneus insolens Adams	1		
INSECTIVORA:			
Ictops dakotensis Leidy	3		
RODENTIA:			
Ischyromys typus Leidy	7*		
Eumys elegans Leidy		1†	
Palæolagus haydeni Leidy	6		
Palæolagus turgidus Cope	2		
PERISSODACTYLA:			
Hyracodon arcidens Cope	2		
Hyracodon nebrascensis Leidy	1		3
Hyracodon apertus Sinclair	1		2
Cænopus occidentalis Leidy	2		
Colodon occidentalis Leidy	1		
Colodon dakotensis Osborn and Wortman	1		1
Mesohippus bairdii Leidy	29		5‡
ARTIODACTYLA:			
Archæotherium mortoni Leidy	12		1
Archæotherium crassum? (Marsh)	1		
Archæotherium wanlessi Sinclair	2		
Archæotherium sp.			1§
Perchœrus nanus (Marsh)			1
Agriochœrus antiquus Leidy	1		
Oreodon culbertsoni culbertsoni Leidy	32		
Oreodon culbertsoni periculorum Cope	3		1
Oreodon gracilis Leidy	3		1
Eporeodon bullatus Leidy	2	1‖	2
Leptomeryx evansi Leidy	6		1¶
Poebrotherium andersoni Troxell	1		
Poebrotherium sp.	5		1
AVES:			
Birds' eggs	2		2**

* One specimen from clays just above lower zone of nodules is here included

† Specimen of *E. elegans* was found 30 feet above lower zone of nodules.

‡ One specimen here included from clays about 10 feet (plus or minus) below upper zone of rusty nodules.

§ Very large astragalus of an entelodont 6 feet below upper zone of rusty nodules.

¶ About 10 feet below upper zone of rusty nodules.

‖ 30 feet above lower zone of rusty nodules.

** Clays about 10 feet (plus or minus) below upper zone of rusty nodules.

REPTILIA:		
Peltosaurus granulosus Cope	1	
Stylemys nebrascensis Leidy	16	2

III. CRITIQUE ON THE FAUNAS.

The various elements of the faunas of the two concretionary zones may now be examined in detail in the order listed.

CREODONTS: *Hyænodon.*

Our collections of the past three summers are exceptionally rich in remains of *Hyænodon* which, strange to say, exceed numerically those of the Canidæ. We are particularly fortunate in having secured a number of uncrushed and exceptionally fine skulls, a series culminating during the summer of 1922 in a magnificent, complete and uncrushed skull of *Hyænodon horridus* with articulated lower jaws, found by Mr. T. B. Lawler, whose recent untimely death is to be deeply regretted. These specimens illuminate certain points in cranial anatomy which have been regarded as important in classification.

Mr. Thorpe[9] has recently sought to establish a new genus *Neohyænodon* for the reception of Leidy's *Hyænodon horridus,* proposing generic characters as follows: "Larger than *Hyænodon,* dolichocephalic, glenoids far below the basicranial plane, basicranial region foreshortened, dentition similar to *Hyænodon* except for the antero-external buttress on the paraconid of $m_{\overline{3}}$." The absence of this buttress in *H. cruentus* has been asserted by Osborn and Wortman[10] and also by Scott,[11] but I find it present, although not prominent, on $m_{\overline{3}}$ on both sides of the lower jaws of the specimen Professor Scott figures as *cruentus* in the memoir referred to (No. 10010 Princeton University Geological Museum), as well as in several other specimens which I cannot separate from *H. cruentus.* It is also present in *H. crucians,* the type specimen of *H. mustellinus,* the type of *H. paucidens,* and in such European representatives of the group (*H.*

[9] Thorpe, M. R., "A New Genus of Oligocene Hyænodontidæ," *Amer. Jour. Sci.,* Fifth Series, Vol. III., No. 16, April, 1922, pp. 277–287.

[10] Osborn, H. F., and Wortman, J. L., *Bull. Amer. Mus.,* Vol. 6, 1894, Table on p. 224.

[11] Scott, W. B., *Jour. Acad. Nat. Sci. Phila.,* Vol. IX., No. 2, 1886, Table on p. 175.

vulpinus) as I have been able to examine. I have also observed it on $m\overline{2}$ in the type of *H. mustellinus* and in unworn teeth of *H. crucians, H. cruentus,* and *H. vulpinus,* with faint traces of it on unworn $m\overline{1}$ in both of the latter, and would expect to find it on the unworn anterior molar of the other species. The shortening of the basicranial region and elongation of the glenoid pedicel seem to me of specific rather than generic significance. Our newly acquired skull of *horridus* is an old individual, with worn teeth and interlocked jaws, and measures 320 mm. from prosthion to occipital condyles, slightly exceeding in this respect the larger of the Yale specimens. The interlocking of the jaws conceals the lower teeth, but the buttress is plainly seen in little-worn teeth in another lower jaw of similar size (No. 12649). Here the tip of the mandibular angle is decidedly inverted on both sides, as it is also in our large skull, whatever may be the situation in the Yale specimen.

Fortunately, the auditory bullæ are preserved on both sides in a splendid skull of *Hyænodon cruentus* (No. 12580 Princeton University Geological Museum) found by Mr. H. R. Wanless during the summer of 1920. The bulla on the left side is in normal position, and is shown, natural size, in Fig. 1, where it is seen to be a half ring, circular in cross section, the gap in the rim opening outward and forward, and the whole lying loosely above the deep cavity lodging the petrous. I have excavated the auditory region in our

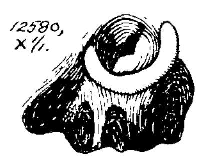

FIG. 1. *Hyænodon cruentus,* No. 12580. Auditory region of the left side seen from below, natural size, showing the semi-circular, loosely attached, tympanic bulla lying above the petrous.

large skull of *H. horridus,* and find that it agrees structurally with the same region in *H. cruentus,* the ring-like bulla, however, being missing, as usual. The petrous crosses obliquely a large deep fossa similar to that found in the European hyænodons, specimens of which in the Paris museum, Père Pierre Teilhard de Chardin has kindly examined for me and on which he reports as follows:

"J'ai cherché sans résultat sur les spécimens du Muséum le détail anatomique que vous désirez connaître pour les Créodonts (Hyænodontids et formes voisines). Nous possédons quatre bases de' crâne bien conservées, deux d'*Hyænodon,* une d'*Pterodon,* une d'*Quercytherium.* Aucune ne possède, ou n'a conservé, d'appareil ossifié, même annulaire, autour de la cavité tympanique. Sur *Hyænodon,* le rocher ('petrosum') est dans une fosse profonde qui suppose l'existence d'une enveloppe, au moins cartilagineuse. Mais, sur les fossiles, cette enveloppe a disparu ou s'est détachée, sans laisser de trace."

Undoubtedly some sort of soft tissue supported the slender semi-annular tympanic, roofing over the ragged tympanic cavity as suggested by Père Teilhard. With its decay, the ring-bulla readily became detached. In fact, on the right side of No. 12580 it occupies practically a vertical position, and has not been observed at all in any other skull in our collection.

Great variation occurs in the extent to which the posterior nares are roofed in by the palatines and pterygoids both in *H. crucians* and *H. cruentus.* In some specimens a deep narrow notch extends forward between the palatines, well anterior to the suture between the latter and the pterygoids, while in others the palatines are in contact throughout, or but slightly notched posteriorly, and only the pterygoid processes separated by a notch. Confining the census to our new material, this deep notching occurs in one of four specimens of *H. cruentus* and in two out of four specimens of *crucians,* and renders doubtful the specific distinctness of *H. leptocephalus* Scott,[12] in which the palatines are described as "in contact throughout; pterygoid plates of alisphenoids meeting below." One specimen showing the juncture of the pterygoids posterior to the suture is noted by Thorpe in the Yale collection, so that this variation is fairly common, although less so than the others. If *leptocephalus* is retained, then both the deeply notched and shallow notched variants of *crucians* and *cruentus* are entitled to names, which seems like forcing matters unduly.

Both *H. paucidens* and *H. mustellinus* are Oreodon beds forms, the latter from the lower Oreodon beds in Corral Draw (not lower

[12] Scott, W. B., *Bulletin of the Mus. of Comp. Zoölogy, Harvard,* Vol. XIII., No. 5, p. 152, Fig. 1.

Protoceras beds, as announced in the type description),[13] and, to judge from the matrix, probably from the lower concretionary zone.

Apart from these two forms, there seems to be little tendency towards the development of transitions in size between the three common species, which form categories well separated in size, with the exception of our No. 12539, which is not separable from *H. cruentus* by any character other than size, but is distinctly smaller than the average in that species, although much larger than *crucians,* as will appear from the following measurements:

	H. crucians. No. 12540.	*H. cruentus.* (small). No. 12539.	*H. cruentus.* No. 12521.	*H. cruentus.* No. 12580.	*H. cruentus.* No. 10995.	*H. cruentus.* No. 10010.
Length p$\underline{4}$–m$\underline{2}$ ...	73.5 mm.	99 mm.	114 mm.	109 mm.	106 mm.	107 mm.
" p1–4 ...	48	62.5	68	71	67.5	67
" m$\underline{1}$–$\underline{2}$ ..	25.5	36.5	46	38	38.5	40
" p4...	12	16.5	—	19	20	18
" m2......	14	22	29	24.5	24	23

DOGS: *Daphænus and Cynodictis.*

I have experienced considerable difficulty in formulating conclusions regarding the species to which our specimens of the larger dogs of the genus *Daphænus* should be referred. The differences involved, and the melange of characters in the various specimens, all of which are contemporary and from the lower concretionary zone, seem to be comparable to the case of the smaller entelodonts of the genus *Archæotherium* discussed on a former occasion and referred to on a later page of the present paper. Possibly a similar explanation will apply here. For reasons which will appear later, I have adopted the expedient of referring all of them to Leidy's *D. vetus.*

Taking dental characters first, the newly acquired Princeton material presents an interesting series of contemporary variations shown in Fig. 2. Some of these cannot be fitted into the existing classification. Among them, our No. 12587, Fig. 2, in the length of

[13] Scott, W. B., "The Osteology of *Hyænodon*," *Jour. Acad. Nat. Sci., Phila.,* Vol. IX, p. 500.

the upper molar series and in the dimensions of their crowns, approaches closely Leidy's fragmentary type of *Daphænus vetus*,[14] where the last molar " has a small transversely oval crown with two tubercles and is inserted into the jaw by a single fang," [15] which agrees closely with the situation found in our No. 12587 except that in it the outer tubercle is smaller than the inner, while in Leidy's

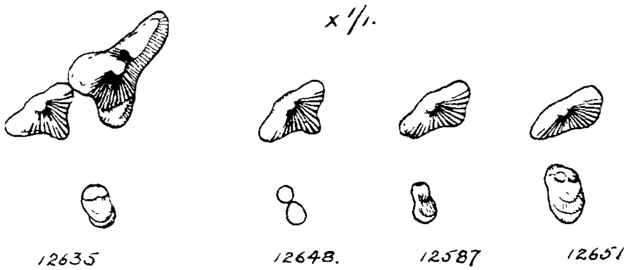

FIG. 2. *Daphænus vetus* Leidy. Comparative series of p3 (upper row) and associated m3 (lower row) to show variation in crown pattern, natural size.

type, both seem to have been alike. The latter lacks all the anterior teeth including p3 which, in our specimen, shows the posterior half of the crown bulged inward broadly at the base. A second variant, also with small single-rooted third molar (No. 12635, Fig. 2), has this inward bulge greatly intensified and centrally placed opposite the main cusp, not supported by a separate root, but by a widening of the posterior root of p3. Here m3 shows a small posteroexternal cusp, so that the outer part of the crown is wider than the inner. A similar maximum bulging of the inner side of p3 is found again in our No. 12648, Figs. 2 and 3, but here m3 was double-rooted, the alveolus for the inner fang being larger than the outer. The dimensions of its crown probably did not exceed those of No. 12635. In the matter of double-rooted third molar, and in other respects to be indicated later, it agrees with Hatcher's *Proamphicyon nebrascensis* which, to judge from Hatcher's figure,[16] lacks all trace of internal

[14] Leidy, J., " The Extinct Mammalian Fauna of Dakota and Nebraska," Pl. I., Fig. 5.

[15] Ibid., p. 34.

[16] Hatcher, J. B., " Oligocene Canidæ," *Mem. Carnegie Mus.*, Vol. I., No. 2, p. 98, Fig. 7.

bulge in p3. Finally, a fourth variation appears in No. 12651, Fig. 2, where m3 has two outer cusps, is quite large and apparently single-rooted, and the shape of p3 agrees closely with Hatcher's figure of the specimen referred by him to *Daphænus felinus*.[17] In only one of our new specimens, No. 12635, is the canine retained undamaged, and is there long and compressed with posterior cutting edge, as in *P. nebrascensis* Hatcher. In all, the upper sectorials are of the type shown in Fig. 2, No. 12635. In Fig. 3 the external concavity of their shearing blade has been somewhat masked by fracture of the tooth crown and displacement of the broken parts by matrix. If these differences in tooth structure are to be regarded as specific, certainly one, and perhaps two, new species will have to be admitted, for neither No. 12635 nor No. 12648 agree with anything previously described, and differ from each other in the number of roots supporting m3.

I have also attempted, without success, to use the structure of the auditory region for classification purposes. This area is exceptionally well preserved in many of our new specimens and might be assumed to afford constant characters. These large daphænids undoubtedly had a loosely attached auditory bulla, now lost in all our material. Complete bullæ are preserved in several specimens of the smaller species, *D. hartshornianus,* in the American Museum collection and a partial bulla was figured by Professor Scott in his memoir on the White River Canidæ.[18] This figure is indefinite in the details of the mastoid region, which has, subsequently, been more elaborately figured by Hatcher,[19] but unfortunately from a distorted specimen. In Fig. 3 I have attempted to show it as seen in an undistorted and uncrushed skull lacking the bullæ, No. 12648, from the lower concretionary zone in Corral Draw. Comparison with Hatcher's figure just referred to will show a number of striking differences. With regard to these, Mr. O. A. Peterson informs me that the condylar foramen in Hatcher's drawing is represented too far forward, and that crushing seems to have affected the size and probably the shape of the

[17] Loc cit., Pl. XVI., Fig. 5.

[18] Scott, W. B., "Notes on the Canidæ of the White River Oligocene," *Trans. Am. Phil. Soc.,* Vol. XIX., Pl. XIX., Fig. 4.

[19] Loc. cit., p. 74, Fig. 3.

reniform fossa, which is smaller and of different shape on the left side, "a feature not as faithfully indicated in the illustration referred to as might have been," and other errors are involved in his Pl. XVI., Fig. 5, which, so far as the basicranial foramina and the tympanic region are concerned, "is not to be too much relied upon." Similar lettering has been used for the cranial foramina in Fig. 3 to facilitate comparison with Hatcher's drawing. Fortunately, such

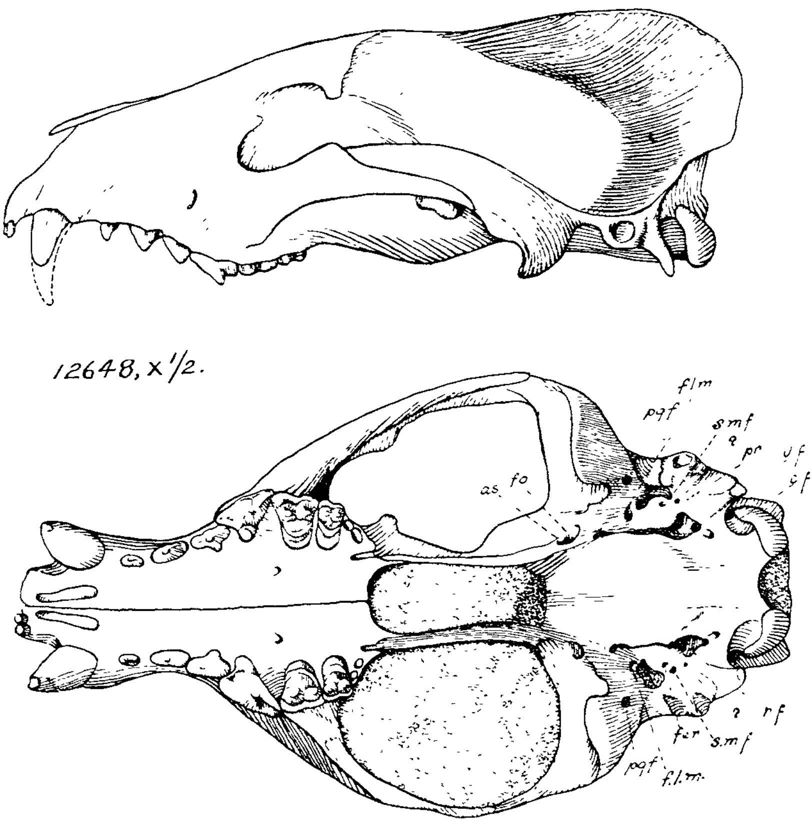

FIG. 3. *Daphænus vetus* Leidy, No 12648. Side and palatal views of uncrushed skull, one half the natural size, drawn from photographs and the specimen. *as*, Alisphenoid canal; *fo*, foramen ovale; *pg. f.*, post-glenoid foramen; *f.l.m*, supposedly the foramen lacerum medium; *fe. r.*, fenestra rotunda (the fenestra ovalis is not visible in palatal view in this skull, but lies in the dark shadow on the outer side of the petrous, where there are two openings on the left side and one on the right); *s.m.f.*, stylo-mastoid foramen; *?*, small circular foramen perforating mastoid; *pr.*, petrous; *j.f.*, jugular foramen; *r.f.*, reniform fossa; *c.f.*, condylar foramen.

of our new specimens as retain the auditory region show no crushing, and comparison may be made directly with them. In No. 12648, illustrated in Fig. 3, the reniform fossæ are small, and the broad expanse of mastoid surface back of the petrous promontory and between the mastoid and paroccipital processes is unbroken, except for a small, circular foramen, (?) in my drawing, and also shown, but not named, in Hatcher's figure. In most specimens of *Daphænus*, both large and small, this region is penetrated by a large, irregular, often smooth-lipped vacuity. A transitional condition is found in

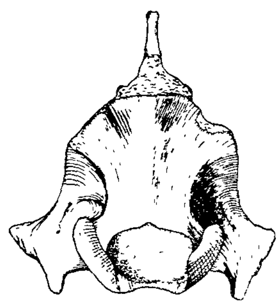

FIG. 4. *Daphænus vetus* Leidy, No. 12648. Back of the skull, one half natural size, showing the high sagittal crest.

our No. 12587, which, as will be recalled, more closely resembles the type of *D. vetus* than do the others, and which shows, on the left side, a small foramen perforating the mastoid, as in No. 12648, while on the opposite side there is a considerable vacuity replacing the foramen. The reniform fossæ also vary greatly in shape in the different specimens, and I am forced to the conclusion that little dependence can be placed on the structure of the auditory region in the matter of species making.

In the complete and uncrushed skull shown in Figs. 3 and 4, the sagittal and occipital crests are seen to be high and sharp, as in Hatcher's *Proamphicyon nebrascensis*[20] which, by the way, does not seem to me worthy of a generic rank separate from *Daphænus*, but to present characters no farther removed from it than those discussed above under dentition. Both canines in the specimen figured were

[20] Loc. cit., p. 96, Fig. 6.

weather-damaged previous to fossilization, and I have increased their length, in outline, to approximate those of No. 12635 already described.

It is perfectly obvious thus far that differences between individuals multiply as more specimens become available, and the possibility of adding to the nomenclature seems limitless. I am therefore disposed to expand our conception of *D. vetus* Leidy to include these various (probably intercrossing) strains, among them *Proamphicyon nebrascensis* Hatcher, which, by the way, is from the Oreodon beds.

When a series of lower jaws is examined, intergradations are noticeable in the size and strength of the mandibular ramus, the sinuosity of its ventral border, and the degree to which the posterior lower molar and the anterior border of the ascending ramus are approximated, as also in the size of the teeth themselves, which inclines me to question the possibility of separating species on these grounds, and so of distinguishing between *D. vetus* Leidy and *D. felinus* Scott.

I have no new data to offer regarding *D. hartshornianus* Cope which, by some chance, is represented in our new collections by but a single specimen of a rather small individual from the lower concretionary zone. I am not convinced of the desirability of referring it to the genus *Protemnocyon* as proposed by Hatcher.[21]

No additional light is thrown on *Cynodictis* by the collections of 1920 to 1922. Earlier Princeton expeditions have found the common species, *Cynodictis gregarius* Cope, represented in the Oreodon beds by a comparatively large number of specimens, usually skulls. By some chance, only one fairly complete skull, with attached lower jaws, and two other very fragmentary skulls, were secured by us during the past three summers of field work. All are from the lower concretionary zone and from the Corral Draw-Little Corral Draw area.

CATS: *Dinictis and Hoplophoneus.*

All specimens referable to *Dinictis* in our 1920–22 collections are from the lower noduliferous zone, and fall naturally into two distinct and sharply contrasting size groups, identified as *Dinictis felina*

[21] Loc. cit., p. 104.

and *Dinictis squalidens* respectively, the former, as indicated in the list, represented by a larger suite of specimens; the latter, by a single specimen, No. 12573, from the lower concretionary zone in the drainage basin of Bear Creek, 5.1 miles south of Scenic, along the Reservation road. This individual was somewhat smaller in tooth dimensions than the specimen (No. 11379 Princeton University Geological Museum) referred by Adams to *squalidens,* as indicated in the appended table of measurements, but I do not think these sufficient to affect the specific reference.

	No 12573.	No 11379.
Upper premolar—molar series, length	37 mm.	41 mm.
Upper sectorial, length anteroposteriorly	16	18.5
Lower premolar-molar series, length	44.5	48

Apropos of Mr. Thorpe's [22] identification of *Pogonodon* in the White River (*Pogonodon cismontanus*), it may be of interest to note that one of the above-mentioned specimens of *Dinictis felina,* No. 12698, a lower jaw with back of skull, has no trace whatever of the metaconid on the right sectorial molar, and only a small metaconid on the opposite side. $M\overline{1}$ is still retained on both sides, although small and crowded completely to the inside of $p\overline{4}$ on the right side, overlapping the latter tooth. Its loss would convert the right side of this jaw into a *Pogonodon,* in which direction, apparently, some individuals of *Dinictis felina* were varying.

A number of finely preserved skulls of hoplophonids of intermediate size from the lower concretionary zone, obtained principally by the 1922 expedition, make possible the recording of a larger series of measurements than has hitherto been available. The genus is so well known that I have not attempted detailed drawings, and merely present outlines in Figs. 5 and 6, to give the general proportions of two of the skulls which I desire to discuss, depending on the table of measurements for exact dimensions, where figures for two skulls in the Yale collection, taken by Mr. Thorpe [23] as the types of *Hoplophoneus latidens* and *Hoplophoneus molossus,* have been placed in parallel columns with similar measurements of five skulls in the

[22] Thorpe, M. R., "New Species of Oligocene (White River) Felidæ," *Am. Jour. Sci.,* Vol. L, Sept., 1920, p. 222.

[23] Thorpe, loc. cit., pp. 207–224.

Princeton collection, of which No. 11013 is the one used by Adams [24] in further defining *Hoplophoneus primævus*. *H. molossus* Thorpe is smaller in skull length, tooth row and various other measurements than the other skulls in the series, although exceeding certain of them

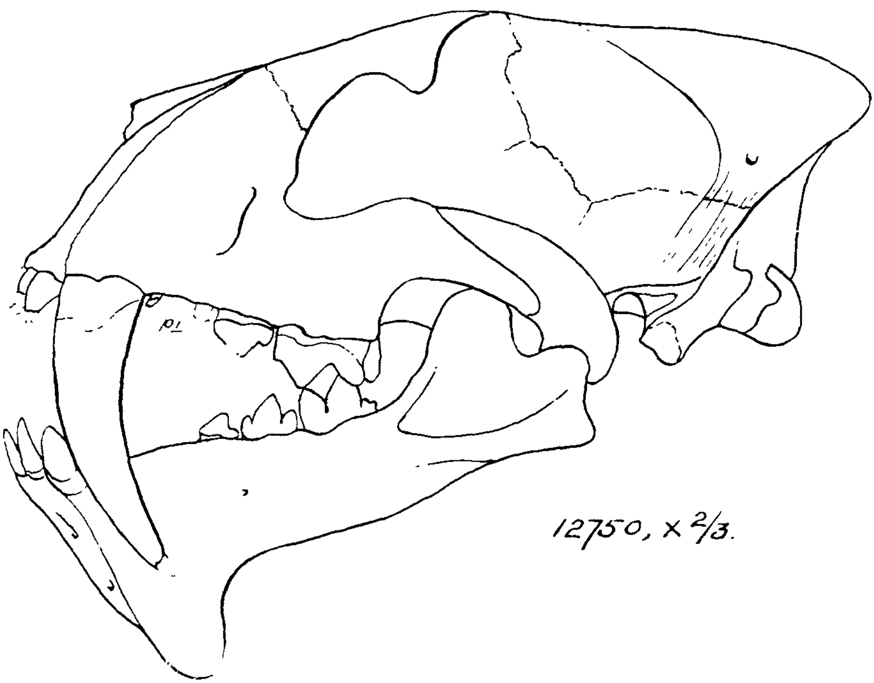

FIG. 5. *Hoplophoneus primævus* Leidy, No. 12750. Side view of the skull and jaws two thirds the natural size. P3–m1 have been outlined from the opposite side. Drawn from a photograph and the specimen.

in various respects, while our No. 12749 (Fig. 6) is, in general, larger. The others, including *H. latidens* Thorpe, agree so closely in size with Adams' specimen of *H. primævus* and with the smaller of the two skulls figured by Leidy on Pl. 4 of the Ancient Fauna that I see no reason for referring them elsewhere than to that species.

When arranged serially with reference to skull lengths, this newly acquired Princeton material is found to intergrade in size between *H. primævus* and *H. robustus*, as follows:

[24] Adams, G. I., *Amer. Jour. Sci.*, 1896, pp. 419–444.

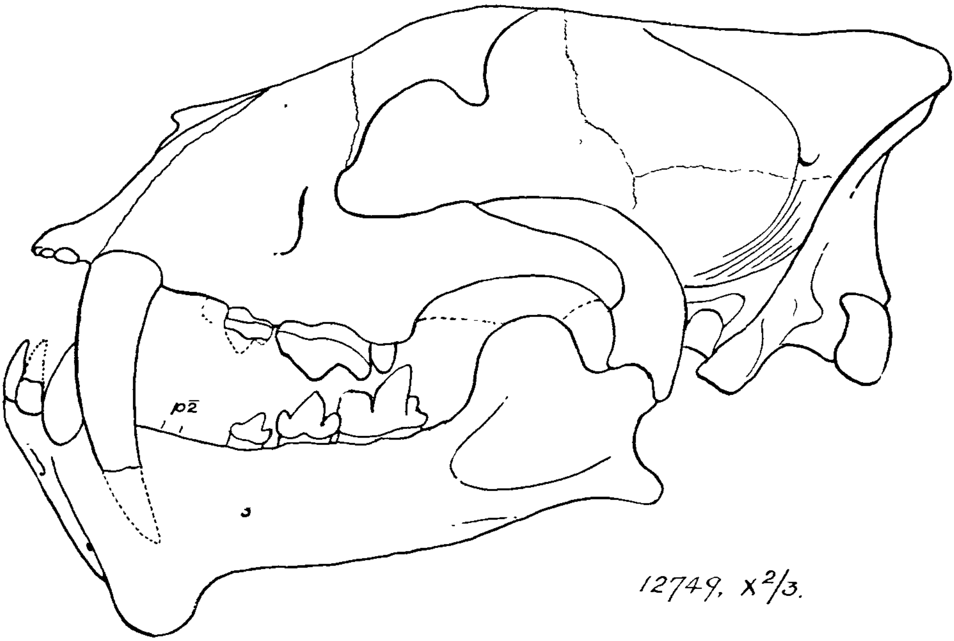

FIG. 6. *Hoplophoneus robustus* ? Adams, No. 12749. Side view of the skull and lower jaw, two thirds the natural size. Center of arch reconstructed. Drawn from a photograph and the specimen.

H. oreodontis Cope, length pmx.-condyles	135 mm.	*
H. primævus Leidy, length pmx.-condyles	148	†
H. primævus Leidy, No. 12542, length, pmx.-condyles	154	
H. primævus Leidy, No. 12750, length, pmx.-condyles	157	
H. primævus Leidy, No. 12750a, length, pmx.-condyles	165	
H. robustus ? No. 12749, length, pmx -condyles	174	
H. robustus Adams, length, pmx.-condyles	180	*
H. insolens Adams, length, pmx.-condyles	190	*
H. occidentalis Adams, length, pmx -condyles	260	*

This is precisely what has so often been claimed would happen if a sufficiently large series of specimens was available: the breaking down of specific distinctions and the merging of species with species. In this connection the data presented by A. A. Dunbar Brander in his recent book "Wild Animals in Central India," for size variation in the modern tiger and leopard, are of interest and may well sug-

* Fide Adams; † 151 mm. in No. 11013 Princeton University Geological Museum.

gest caution in the erection of species on a dimensional basis. The leopard seems to be particularly variable in this respect "as mature animals not much bigger than the largest of cats up to animals which bear comparison with small tigers are sometimes met with."

Two of our newly acquired skulls, Nos. 12750 and 12750a, here identified as *Hoplophoneus primævus*, were found less than a foot apart, intermingled with parts of both skeletons, in a large nodule of cemented clay-pellet conglomerate in the lower concretionary zone in Battle Spring Draw (Nevis Creek). One of the skeletons has been mounted and is shown in Plate I., Fig. 2. The skulls differ from each other slightly in size, dimensions of the sabre, weight of the supraciliary ridges, depth of frontal depression between the orbits, and rugosity of the mastoids, and I have assumed that the smaller and more perfect skull, shown in Fig. 5 and Plate I., Fig. 2, was perhaps a female. Possibly the pair were mates, drowned together. Differences in the shape of the obturator foramen in the pelves support the idea of a sex difference. Neither skull is at all crushed, although both have required some mending and a minimum of reconstruction from the opposite side to supply missing parts. In the profile, Fig. 5, no reconstruction is involved, except sketching $\underline{p3}$–$\underline{m1}$ from the opposite side. Measurements of a few of the associated limb bones show them to be a little shorter than the figures given by Adams for *H. primævus* (No. 11013 Princeton University Geological Museum) as follows:

	No. 12750.	No. 11013.
Femur	172 mm.	185 mm.
Tibia	139	143
Humerus	153	160
Radius	117	122

Both of these skulls are of interest in showing minor dental anomalies. No. 12750 has an extremely small first upper premolar on the left side, resembling a small incisor, single-rooted, with obliquely placed crown, situated close to the canine and sloping forward, while in No. 12750a, where no trace of this tooth appears, the second premolar is double-rooted on the left side and single-rooted on the right. The dependent flanges in the jaws associated with these skulls are very deep, with round ends, a sort of small-sized

edition of the form I have named *Hoplophoneus mentalis* from the Titanotherium beds.[25] Variations also appear in the shape of the heel of the lower sectorial, which is very small and without posterior basal cuspule in a right ramus, and larger and slightly notched by the development of such a cuspule in what appears to have been the opposite half of the same jaw, as shown in Fig. 5.

Adams' illustrations[26] of the skull of *H. primævus* are misleading in that he has figured a somewhat vertically crushed skull which lacks the top of the head and the sagittal and lambdoidal crests. The dorsal profile of the skull, represented by him, is incorrect, producing a hammer-headed effect quite noticeable in skulls which lack these crests; for example, those shown in Leidy's plate.[27]

A somewhat larger skull, with associated lower jaw, No. 12749, is shown in Fig. 6. The center of each arch has been reconstructed, but otherwise the skull is entire on the side shown in the drawing, and is quite uncrushed, with the margins of the crests unbroken. This specimen, from the lower concretionary zone in Corral Draw, approaches closely in size the type of Adams' *Hoplophoneus robustus,* the larger skull figured by Leidy on Pl. IV., Fig. 1, of The Ancient Fauna, with which it also agrees in a somewhat greater concavity of the facial profile anterior to the orbits and stronger frontal bulge than is found in No. 12750. The lower jaw has a less deep flange than in the latter, although the length of the jaw is greater, the coronoid process has less backward slope, the angle is greater, and in the lower sectorial there is a small basal cuspule in addition to the metaconid, but how constant this might prove to be, in view of the variation noted above in connection with No. 12750, is questionable. An interesting dental anomaly is found in the right ramus of the lower jaw, where a pair of roots, evidently for the support of $\overline{p2}$, are found, 9 mm. in advance of $\overline{p3}$ and occupying a space 4.3 mm. in length. No trace of a corresponding tooth appears on the opposite side where the jaw margin is thin and sharp. I have indicated its position in Fig. 6 by a pair of pointers and the appropriate lettering.

[25] Sinclair, W. J., "A New *Hoplophoneus* from the Titanotherium Beds," *Proc. Amer. Phil. Soc.,* Vol. LX., No. 2, 1921, pp. 96–98.

[26] *Am. Jour. Sci.,* 1896, Pls. X. and XI., opposite to p. 444.

[27] Ancient Fauna, Pl. XVIII., Fig. 1; Extinct Mammalian Fauna, Pl. IV., Fig 5.

MEASUREMENTS.

SKULL.	*H. latidens* type. Yale P. M. No. 10050.	*H. molossus* type. Yale P. M. No. 10052.	*H. primævus.* No. 12542. Princeton.	*H. primævus.* No. 11013. Princeton.	*H. primævus.* No. 12750. Princeton.	*H. primævus.* No. 12750a. Princeton.	*H. robustus?* No. 12749. Princeton.
Prosthion to basion	154 mm.	132 mm.	143 (approx.)	142 mm.	148 mm.	155 (approx.)†	164 mm.
Prosthion to nasion	78.5	75	71.5		74	75	78
Basion to nasion	99	93	94 (approx.)		104	103 (approx.)	112 (approx.)
Prosthion to bregma	126.5	107	109		116	110	121.5
Basion to bregma	71	77	69 (approx.)		76	76 (approx.)	113 (approx.)
Bizygomatic diameter	116	104 (est.)	?		113	116 (est.)	118 (est.)
Diameter postorbital constriction	31	26	23	33	34	32	31
Bimastoid diam. maximum	70.5	60	62	63	66	68	69
Palatal length (prosthion to pterygoid processes of maxillæ)	79	67	73		77	82	80
Post. palatal breadth	47	41	35.5 +		47.5	49.5	47
Inf. orb. margin—alveolar margin (minimum)	24	24	23	19 +	22	22	22
Inf. orb. foram.—post. maxillary margin	28	26	30		30	30	33
Tooth-row, c–m1 inc.	67.5	59	61 right	60	63r. (62l.)	69r.* (63l.)	66 right
" " p3–m1 inc.	36	32	34	34	33	33.5	38 (est.)
Diastema c–p3	16	13	12	11.5	14	14	15
" I3–c	3	1	2 ±	3.5	2.5	4	4.5
Canine	17.5 × 10	15.5 × 7.5	?	15 × ?	15 × 7.5	16 × 8	14 × 8
P3 length of base	11	11	12	11.5	11.5	12	11
" max. width	5	5	?		5	5	5
P4 length of base	21	21.5	18	20	20	19.5	19
" max. width	10	10	9		10	10	11.5

* Lengthened unduly by fracture; † Condyle slightly displaced upward in matrix, shortening basion-nasion and basion-bregma diameters.

MEASUREMENTS (*continued*).

LOWER JAW.	*H. latidens* paratype. Yale P. M. No. 10049.	*H. molossus* type. Yale P. M. No. 10052.	*H. primævus.* No. 11013. Princeton.	*H. primævus.* No. 12750. Princeton.	*H. robustus?* No. 12749. Princeton.
	mm.	mm.	mm.	mm.	mm.
Length, mandibular ramus . .		109 5	118	119	131.5
Depth, " symphysis	40 (est.)	39	38±	40	42.5
" " post. to $p\bar{4}$	21.5	23	22	22	26
" " flange . .	49	41		47	41
Length $p\bar{3}$–$m\bar{1}$ inc.	39	37	34	35	38
Diastema C–$p\bar{3}$ . .	25.5	20	23	25	30
$P\bar{4}$, length of base	14	12.5	12	12	12
" maximum width. . .	6 5	6		6	7
$M\bar{1}$, length of base	17	17	19	15	16
" maximum width	8.5	7.5		7	7.25

Individuals with vestigial teeth similar to those here described serve to bridge the gap between *Hoplophoneus* and *Dinictis*.

Hoplophoneus oreodontis Cope, the type of which, by the way, is from the White River formation of northeastern Colorado and not from the John Day, as stated by Mr. Thorpe,[28] was suggested by Cope [29] as possibly only a regional variety of *H. primævus*. The specimen described and figured by Adams [30] (No. 10515 Princeton University Geological Museum) was obtained by purchase from L. W. Stilwell, and on Mr. Hatcher's index card is entered as from the Oreodon beds, South Dakota Badlands, locality unknown. As is indicated on an earlier page, it constitutes the minimum terminal member of a size variation series, and whether a regional variety of *primævus* or a distinct species, I have not been so fortunate as to find anything referable to it in our recent work, or to Thorpe's *Hoplophoneus marshi*,[31] a small form of about the size of *H. oreodontis* and supposedly from the Oreodon beds.

A large cat, No. 12590, with a skull length (premaxillæ to condyles inclusive) of approximately 191 mm., from the lower nodular zone in Chamberlain Pass, east of Scenic, agrees so closely in this

[28] *Amer. Jour. Sci.*, Vol. L., Sept., 1920, p. 214.
[29] Tertiary Vertebrata, p. 997.
[30] Loc. cit., p. 429, Pl. XI., Fig. 2.
[31] Thorpe, loc. cit., pp. 211–214.

dimension with *H. insolens* Adams that it may be referred there. Just what a series of skulls of some of the larger hoplophonids would show in the matter of size transitions remains, for the present, a matter of interesting speculation.

INSECTIVORES: *Ictops.*

So far as I can see, there are no specific differences between the suites of insectivore specimens from the two levels, nor anything which would warrant their reference elsewhere than to *Ictops dakotensis.* There are some individual differences in size, but that they have specific significance is not apparent.

RODENTS: *Ischyromys, Eumys, Palæolagus.*

Rodents are, undoubtedly, more abundant than the scarcity of specimens in the lists would indicate. Particularly must this be true of the murines (*Eumys* and perhaps others not yet discovered), frequent scorings by their small incisors appearing on all sorts of bones. Possibly *Steneofiber nebrascensis* Leidy should have been added to the fauna of the lower concretionary zone. A fair skull, presented to us by Mr. Walter E. Brown of Interior, was found by his children perhaps in the Lower Oreodon beds on one of the low buttes on his ranch. The small amount of adhering matrix contains a good deal of coarse quartz sand, mica foils, etc. In view of the uncertainty of the specimen's stratigraphic position, I have omitted the name from the lists. It evidently came from the White River series, and the form appears to have been rare.

PERISSODACTYL UNGULATES.

1. *Hyracodon.*

In contrast with the smaller entelodonts, the hyracodons present an orderly contemporary series of progressive mutations, in the direction of complete molarification of the pattern in the upper premolars, something already achieved in $\underline{p2}$–$\underline{4}$ of the milk dentition. Material is not yet available for determining to which one of the four species I have recognized, if species they are, these milk dentitions pertained. So far as the adult dentition is concerned, the changes in premolar pattern discussed and fully illustrated else-

where are consistently alike on opposite sides of the palate in all the specimens which I have examined, not varying irregularly as is sometimes observable in other of the rhinoceroses; and, while the structural changes to which specific names have been applied are small, they are nevertheless in sharp contrast with each other, whereas the size differences within each specific group intergrade by small increments, except in the case of *Hyracodon leidyanus*, here due in all probability to a lack of specimens. This intergradation in size, without change in structure, has led me to discard Mr. Troxell's *Hyracodon selenidens* as a separate species, placing it as the minimum size variant in the *Hyracodon arcidens* series. Our 1922 collection has yielded, from the lower concretionary zone in the Oreodon beds in Quinn Draw, Washington County, a specimen, No. 12765, agreeing closely in size with Mr. Troxell's *H. selenidens* (lengths, upper premolar series 60 mm., upper molar-premolar series 111, lower molar-premolar series 108.5), dimensions which fit in nicely in the table of size variations referred to.[32]

2. *Cænopus.*

I refer our two rhinoceros skulls from the lower concretionary zone to *Cænopus occidentalis* Leidy because they agree in premolar pattern with the skull figured by him on Pl. XII., Ancient Fauna, and referred to this species, which was originally established upon several small fragments of molar teeth. We must not overlook Dr. Leidy's privilege of redefining his species in terms of other and better specimens, and this hautotype skull can hardly be characterized as "not adequate nor dependable" as Mr. Troxell [33] expresses it in his endeavor to substitute *Cænopus copei* for Leidy's classic species. If *Cænopus copei* is "barely distinguishable from *C. occidentalis* except in size" as Mr. Troxell states, our commonly accepted rules of priority do not permit the substitution of the former name for the latter because based on a better specimen. With all due deference to Mr. Troxell's opinion to the contrary, I find Leidy's figure both adequate and dependable for the identification of our newly acquired

[32] Sinclair, "Hyracodon, &c.," loc. cit., p. 68.

[33] Troxell, E. L., "Cænopus, The Ancestral Rhinoceros," *Am. Jour. Sci.*, July, 1921, p. 44.

material of which the best preserved skull, No. 12634, differs from the figure just referred to in having a prominent cylindrical style replacing the cingulum at the entrance to the transverse valley in m3, whereas in Leidy's plate there is a heavy cingulum shown at this point.

3. *Colodon.*

Jaw fragments determined as *Colodon dakotensis* from both the nodular zones are practically identical with specimens already in the Princeton collection, discussed some years ago by Mr. Hatcher.[34] He was in error, however, in his definition of the genus in ascribing to it but two lower incisors. The specimen on which his definition

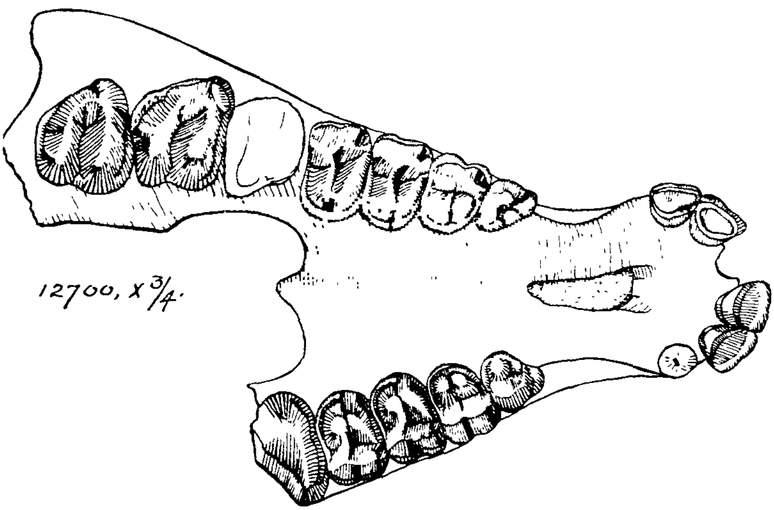

FIG. 7. *Colodon occidentalis* Leidy, No. 12700. Palatal view, three fourths the natural size.

is based, and which he figures with but two incisors, shows part of the alveolus for the third incisor and this tooth is present in the lower jaw of a specimen of the smaller species, *C. occidentalis*, in the collection of the American Museum. No figures of the upper dentition of *C. occidentalis* have been published other than the drawing of *C. procuspidatus* O. and W.[35] which Hatcher referred to *occiden-*

[34] *Am. Jour. Sci.*, 1896, p. 169.

[35] Osborn and Wortman, "Perissodactyls of the Lower Miocene White River Beds," *Bull. Amer. Mus.*, Vol. VII., 1895, p. 365, Fig. 8.

talis. An incomplete palate, No. 12700 Princeton University Geological Museum, from the lower concretionary zone of Quinn Draw basin, Fig. 7, is of a size to fit lower dentitions referred by Hatcher to *Colodon occidentalis* and probably belongs to that form. It is of interest in showing for the first time the width of the palate, the length of the diastema, and all the teeth, $\underline{m1}$ excepted, on either one side or the other. The incisors are horse-like in the presence of a shallow central cup, too shallow to form a "mark" as the tooth wears. The irregular palatal margin on a line with $\underline{p4}$–$\underline{m1}$ is due to fracture, and does not mark the position of the posterior nares.

MEASUREMENTS.

$\underline{I1}$–$\underline{m3}$, length	132 mm.
Diastema, length	20.5
Premolar series, length	41.5
Molar series, length	46

4. *Mesohippus.*

Mesohippus bairdii is certainly the commonest and, to judge from our collections of the past three years, the only species of horse in the lowermost 100 feet of Oreodon beds in the South Dakota Badlands. I am wholly unable to determine at what level the transition from *Mesohippus* to *Miohippus* occurs. All the specimens collected by us in the upper concretionary zone, while comparatively few in number, are indistinguishable from typical *M. bairdii.*

Although all our horses from the concretionary zones are referable to *bairdii,* it by no means follows that all accord strictly in dental pattern with the type of that species, as the series of unworn, or but little worn, third molars, illustrated in Fig. 8, will show. These specimens which differ strikingly in the shape of the hypostyle, development of rudiments of the crista, forking of the tip of the mataloph, etc., are all from the lower concretionary zone, and come as close as possible to being a contemporary assemblage. I have confined myself to the third molar, not because it possesses any inherent virtue, but because, being the latest erupted, it is unworn in many specimens and shows details soon obliterated by use. The hypostyle in the molars of *Mesohippus* is a recently acquired character, appearing first in certain Titanotherium beds species, and,

in *bairdii*, it still seems to be seeking for expression, not having settled down to a fixed policy, as shown in Fig. 8, where some, not necessarily all, of the different forms assumed by this cusp, in combination with other features, are illustrated.

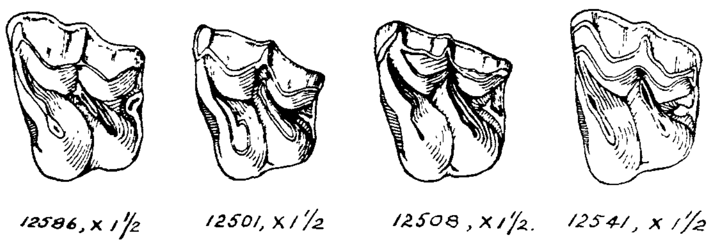

FIG. 8. *Mesohippus bairdii* Leidy. Series of upper m3 to show variations of hypostyle, etc. One and one half times the natural size. Specimens in the Geological Museum of Princeton University.

In No. 12586, left m3, Fig. 8, the hypostyle is cupped by the development of a pli-hypostyle enclosing an elongated depression. There is a rudimentary crista and the tip of the metaloph is forked to form a small pli prefossette and pli postfossettte, but the cup of the hypostyle may be nearly circular, and the tip of the metaloph unforked, as in our No. 12572.

In No. 12501, left m3, Fig. 8, the hypostyle is a sharp spur-like prolongation of the cingulum, extending forward with no trace of cupping. The tip of the metaloph is unforked and separate from the ectoloph and there is the rudiment of a crista but the same type of hypostyle may be associated with slight forking of the end of the metaloph (left m3 in No. 12505), or the hypostyle may develop a smaller spur than in No. 12508, Fig. 8, the other structures remaining unchanged (right m3 No. 12505).

In No. 12508, Fig. 8, where the right m3 is drawn reversed, the hypostyle has a prominent outwardly directed spur, and the metaloph is confluent with ectoloph by a narrow bridge. In this tooth the points of the para and metaconids have been damaged by fracture.

In No. 12541, left m3, Fig. 8, and other teeth of similar pattern, the hypostyle is a triangular area with irregular rugosities, wearing to a truncated cone, sometimes joined by a narrow isthmus to the rest of the posterior cingulum. The metaloph is sometimes fused with the ectoloph, sometimes not.

If these are to be regarded as individual variations, as seems probable in view of the close agreement of all the specimens in size and structure with *M. bairdii* and their close contemporaneity, all being from the lower concretionary zone, it would seem probable that *M. trigonostylus* Osborn, which is based on a specimen with hypostyle comparable to No. 12508, Fig. 8, will have to be regarded as conspecific with *bairdii*. The type of this species is "from the Metamynodon sandstones in the middle Oreodon beds." [36] As shown in Mr. Wanless' paper, the Metamynodon channels all lie below the upper concretionary zone. A second form, *Mesohippus obliquidens* Osborn, the type of which is "from the nodular layer in lower middle layer of the Oreodon beds; specimen covered by ferruginous oxide," [37] has long since been regarded by Dr. Matthew as conspecific with *bairdii*, so that the latter, as already intimated, seems to stand alone as the only equine species in the lower Oreodon beds so far as the South Dakota area is concerned.

Artiodactyl Ungulates.

1. *Archæotherium.*

Heavy rains during the winter and spring of 1921–22 washed out from the loose surface clay the missing tip of the left cheek flange pertaining to the rather remarkable entelodont skull No. 12624, collected by Mr. H. R. Wanless during the previous summer from the clays of the lower concretionary zone at Culbertson's locality on Bear Creek, northeast of Scenic in Pennington County. This specimen I have already described and figured, referring it to *Archæotherium mortoni*.[38] A corrected figure, showing the complete cheek flange, is presented herewith, Fig. 9. If taken by itself, this specimen would constitute a perfectly good species differing from the majority of the specimens of *A. mortoni* in skull length, size of the cheek flange, molar pattern, and premolar spacing. Only by a wealth of contemporary material has it been possible to work out the anastomosis of characters described in the paper just referred to, which has led me to visualize *A. mortoni* as made up of

[36] "Iconographic Revision," p. 47.

[37] Ibid., p. 48.

[38] *Proc. Amer. Phil. Soc.*, Vol. LXI., p. 53 et seq.

a number of interbreeding strains which differ from each other by various small unit characters, singly or in combination. It is entirely possible that the same conception should be applied to certain of the larger species of the genus, thus far described from the Oreodon beds, and that the matter may be further complicated by sexual dimorphism affecting the shape of such excrescences as the cheek flanges and chin processes, but adequate data in the shape of a large suite of contemporary specimens of the larger forms are not yet available for discussion.

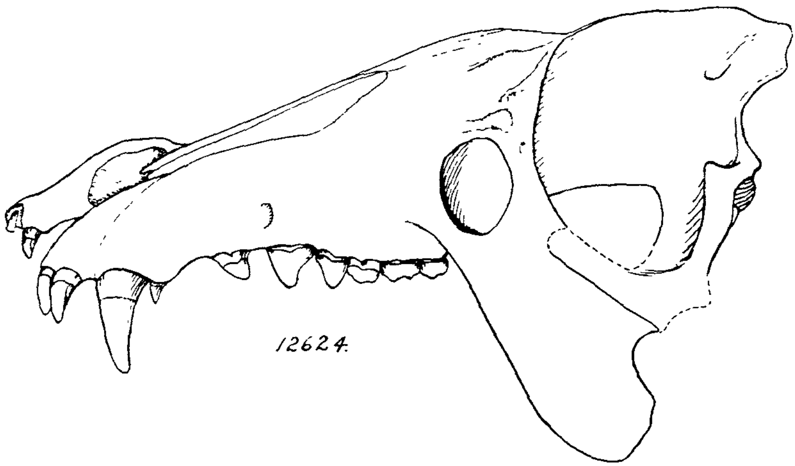

FIG. 9. *Archæotherium mortoni* Leidy, No. 12624. Corrected drawing showing the complete cheek flange, about one fifth the natural size. View not in absolute profile.

Thus far *Archæotherium* is the only entelodont genus found in the Titanotherium and Oreodon beds, but probably occurs also in the Protoceras-Leptauchenia beds, as indicated by a fine skull from this level in the collection of the Field Museum, no description of which has yet been published. *A. mortoni* is certainly common to the Titanotherium and Oreodon beds, as also seems to be true of the large form for which I have proposed the name *A. scotti*,[39] and which I think is represented by a somewhat crushed skull and jaws, No. 572, in the American Museum, collected by their 1882 Expedition from the upper Oreodon beds. As revised to date, the list of entelodont species from the Oreodon beds reads as follows:

[39] *Proc. Am. Phil. Soc.*, Vol. LX., No. 4, 1921, p. 468 et seq.

1. From the Lower Concretionary Zone:
   A. mortoni Leidy and its variants.
   A. crassum ? Marsh.
   A. wanlessi Sinclair.
2. Probably from the Lower Concretionary Zone, and certainly from the lower Oreodon beds:
   A. ingens Leidy (identified on the basis of the chin processes).
3. Upper Oreodon beds, and presumably lower Oreodon beds also, as the type is from the upper Titanotherium beds:
   A. scotti Sinclair.
4. From the Upper Concretionary Zone:
   A. mortoni.

2. *Perchœrus.*

Fragments of a lower jaw, No. 12736, from the upper concretionary zone on Cottonwood Creek in Washington County, agree so closely in size and structure with the type of Marsh's *Perchœrus* (*Thinohyus*) *nanus*[40] that I have no hesitancy in so determining them. It is a small peccary, smaller than Leidy's *P. probus.*

A few teeth from the upper Oreodon beds in the vicinity of Cedar Pass, near Interior in Jackson County, are probably referable to *P. probus,* with which they agree fairly well in size. The Princeton collection does not contain sufficient peccary material from definitely known horizons in the Oreodon beds to work out the range in time of the above species. Their presence or absence in the nodular zones is probably controlled largely by the chances attending the collection of rare material, but may reflect differences in local habitat.

3. *Agriochœrus.*

No new data are available regarding the stratigraphic range of the two known species of *Agriochœrus* from the Oreodon beds. Abundant teeth and other fragmentary materials occur in the lower nodular zone, but complete skulls are rare, only one being secured with the smaller type of distended auditory bulla characteristic of *A. antiquus.* Both *A. antiquus* and *A. latifrons* are represented in the older Princeton collections from the Oreodon beds, but precise stratigraphic data regarding their position in the field are lacking.

[40] *Amer. Jour. Sci.,* Vol. XLVIII., Sept., 1894, p. 271, Fig. 28.

4. *Oreodon and Eporeodon.*

The lower clays of the Oreodon beds, and especially the lower zone of rusty nodules, is the level par excellence of *Oreodon culbertsoni culbertsoni* Leidy, and might well be designated the *Oreodon culbertsoni culbertsoni* zone were it not for the fact that the species is not strictly confined to this level, ranging downward into the upper Titanotherium beds, as shown by a single specimen with the rugged type of bulla characteristic of *O. culbertsoni culbertsoni*, No. 12764 Princeton University Geological Museum, found in place about 18 inches below the contact of the Titanotherium and Oreodon beds, distinctly below the level of the white limestone lens marking the contact in Corral Draw. How far upward it extends into levels above the lower concretionary zone must remain doubtful, for while no specimens of it were found by the Princeton expeditions of 1920–22 in the upper nodular zone of the Oreodon beds or higher, there is a specimen of *O. culbertsoni culbertsoni* in our museum, No. 11080, labeled in Hatcher's handwriting as from the upper Oreodon beds, Corral Draw, and in a matrix which agrees in character with the upper Oreodon clays.

Not all the specimens of this species in which the basicranial region has been prepared in detail agree with the figure given by Osborn and Wortman [41] in possessing separate foramina rotunda. These are completely lacking on both sides in the above-mentioned specimen from the Titanotherium beds, and in another skull, No. 12703, from the Oreodon beds, the foramen rotundum is absent on one side and present on the other. There are also variations in the shape and degree of inflation of the bullæ in individuals which otherwise agree well with *O. culbertsoni culbertsoni* in the characters cited by Osborn and Wortman.

There is, however, never the slightest danger of confusing *Oreodon culbertsoni culbertsoni* with *Eporeodon bullatus* if the auditory region has been preserved. Quite unexpected data regarding the stratigraphic range of the latter are afforded by our collections of the past three years. From the presence of eporeodons (*E. major*) with greatly distended bullæ in the Protoceras beds, and of forms with small rugged bullæ in the Oreodon beds, with a single example of

[41] *Bull. Amer. Mus.*, Vol. VI., 1894, p. 216, Fig. V*A*.

"the transitional form *O. bullatus*" from beds of intermediate position, Osborn and Wortman were led to suggest [42] that the range in time corresponds with the evolution of the bullæ. This, however, is not the case, for typical *Eporeodon bullatus*, agreeing absolutely with Fig. 5C of Osborn and Wortman's paper, occurs in both noduliferous horizons of the Oreodon beds and probably in the Protoceras-Leptauchenia beds as well. The type of *Oreodon* (*Eporeodon*) *bullatus* is from the Titanotherium beds,[43] and the species is tentatively identified by Thorpe [44] from the John Day, certainly the greatest vertical range of any White River form. Incidentally, the finding of *Eporeodon bullatus* on the same level with *Oreodon culbertsoni culbertsoni* has an important bearing on the status of *Merycoidodon* as Professor Scott has brought to my attention. This name was proposed by Leidy in 1848 for an animal represented by the fragment of an upper jaw containing the last two molars, and a second fragment of the lower jaw with the last three molars. Later, other fragments were referred by the same author to *Oreodon* and *Cotylops*. Better material subsequently obtained enabled Dr. Leidy to decide that all three belonged to a single genus for which he expressed the desire to retain the name *Oreodon*, a preference which modern priority purists would have done well to respect, since *Eporeodon bullatus* and *Oreodon culbertsoni culbertsoni* are quite inseparable on the basis of maxillary and jaw fragments lacking the premolars and, with both occurring in the same beds, Leidy's type of *Merycoidodon* might have pertained to either. The name should be permanently discarded.

The smaller form, *Oreodon culbertsoni periculorum*, while far less abundant than *O. culbertsoni culbertsoni*, is found in both noduliferous zones of the Oreodon beds, as is also *Oreodon gracilis*.

5. *Leptomeryx*.

Leptomeryx is represented in both concretionary zones. A lower jaw fragment showing the characteristic structure of the heel of the last lower molar found in *Hypertragulus* was obtained from one of

[42] Loc. cit., p. 219.
[43] "Extinct Mammalian Fauna," pp. 104–5.
[44] *Amer. Jour. Sci*, August, 1921, p. 104.

the Metamynodon channels. Its non-occurrence among our collections from the concretionary zones has probably no other significance than failure to find a specimen of a not too abundant form.

6. *Pœbrotherium.*

The synonymy of the White River camels is badly involved, due to the fact that the first described species from the South Dakota Big Badlands, *Pœbrotherium wilsoni* Leidy, is based on a juvenile specimen retaining the milk premolars and lacking the front of the skull and the anterior teeth, on which specific distinctions within the

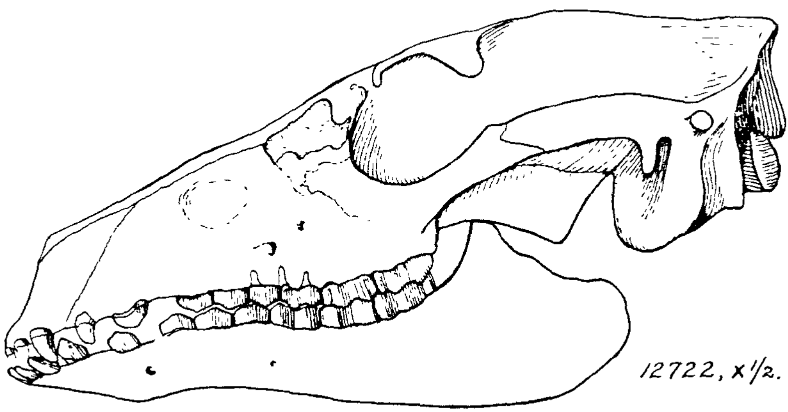

FIG. 10. *Pœbrotherium andersoni* Troxell, No. 12722. Side view of the skull and jaws, one half natural size Arch, pterygoid region and parts of brain case restored. Drawn from a photograph and the specimen.

genus are now based. A second species, *Pœbrotherium labiatum* Cope, from the Cedar Creek beds (Oreodon zone), Colorado, has the type skull mutilated in the same way. So far as the size of the type specimen of *wilsoni* is concerned, it might either be a young *P. labiatum* or *P. andersoni*, if the latter is distinct from *labiatum*. Juvenile individuals of *P. eximium* are much smaller than the type specimen of *wilsoni*, while the adults, which have been confused with *wilsoni* in the older literature, may be readily separated from the specimen here figured by differences in the shape and spacing of the anterior teeth.[45]

[45] See *Bull. Amer. Mus.*, Vol. X., 1898, p. 111, Fig. 7, now referred to *P. eximium* Hay.

I am referring provisionally to *P. andersoni* Troxell, the specimen shown in Fig. 10, No. 12722 Princeton University Geological Museum, from the lower concretionary zone in the west part of Indian Creek basin, near the pass into Little Corral Draw, Washington County, South Dakota. It is a fully adult, uncrushed skull, a little larger than the type of *andersoni* with which it agrees closely in structure, having $i\underline{3}$ large and caniniform and $c\underline{1}$ small. No trace of an internal basal pillar, however, appears between the inner lobes of $m\underline{3}$. The preorbital vacuity, not indicated in Mr. Troxell's figure of *P. andersoni*,[46] but present in his type specimen, has about the same extent as No. 12722 here figured, making allowance for the slightly smaller size of the former specimen and its deformation by crushing. Whether our No. 12722 could be separated specifically from *wilsoni*, if mutilated to the same extent as the type of that species, is doubtful. It does not differ markedly in size from the type of *P. labiatum*, but has $p\overline{2}$ closer to the posterior end of the symphysis (*i.e.*, a shorter gap between $p\overline{1}$ and $p\overline{2}$). Just how much dependence should be placed on this point is doubtful in view of the variation observable in another of our specimens of similar size, No. 12725, also from the lower concretionary zone as exposed in Little Corral Draw northwest of Cottonwood Pass. In the latter the gap between $p\overline{1}$ and $p\overline{2}$ measures 5.5 mm. in contrast with 9 mm. in No. 12722. These specimens further differ in having the lower canine quite small in cross-section in the former and considerably larger in the latter, as indicated in the figure, perhaps a sexual difference. I am inclined to suspect, on the basis of comparable size in skeleton, that the specimen described and figured some years ago by Professor Scott as *P. labiatum*[47] (No. 10527 Princeton University Geological Museum) might, with equal propriety, be referred to *P. andersoni*. An examination of Cope's type specimen of *labiatum* shows that the lower margin of the jaw is incorrectly drawn in his illustration.[48]

[46] Troxell, E. L., "An Oligocene Camel, *Poebrotherium andersoni*, n. sp.," *Amer. Jour. Sci.*, Vol. XLIII., May, 1917, pp. 381–389, Fig. 1.

[47] Scott, W. B., "On the Osteology of *Poebrotherium*, A Contribution to the Phylogeny of the *Tylopoda*," *Jour. of Morphology*, Vol. V., 1891, p. 1 et seq.

[48] Cope-Matthew, "Hitherto Unpublished Plates of Tertiary Mammalia and Permian Vertebrates," Pl. CXV., Fig. 1.

It should be as indicated in Fig. 10 and as in Leidy's figure of his type specimen of *P. wilsoni.* I do not find any striking differences in the shape of the otic bullæ between Cope's specimen of *labiatum* and our *P. andersoni.* They are a little rounder in the former than in the latter, but contemporary specimens of comparable size from the lower nodular zone vary in the degree of bulla inflation. Cope's figure shows $m\overline{3}$ set well in front of the ascending ramus, whereas in No. 12722 this tooth encroaches more closely on the ramal margin.

Measurements.

Maximum skull length, premaxillæ to supraoccipital, inc. .... 201.5 mm.

Length $p\underline{2}$–$m\underline{3}$	63.5 mm.	$P\underline{4}$ anteroposterior diameter	8
Length $p\overline{2}$–$m\overline{3}$	69.5	$M\underline{1}$ anteroposterior diameter	10
Length entire inferior dentition	110.5	$M\underline{2}$ anteroposterior diameter	14
		$M\underline{3}$ anteroposterior diameter	15
		$P\overline{1}$ anteroposterior diameter	5.6
P1 anteroposterior diameter	8	$P\overline{2}$ anteroposterior diameter	8.6
$P\underline{2}$ anteroposterior diameter	9	$P\overline{3}$ anteroposterior diameter	9 5
$P\underline{3}$ anteroposterior diameter	9 2	$P\overline{4}$ anteroposterior diameter	9

P. eximium occurs in the lower Oreodon beds, as shown by a juvenile specimen, No. 11011 Princeton University Geological Museum, collected in 1894 by Mr. H. F. Wells in Corral Draw, presumably from the lower concretionary zone. It is not identifiable among the specimens secured by us during the past three years, and has, accordingly, been omitted from the list.

Birds' Eggs.

A remarkably large number of fossil eggs has been obtained by us during the last three summers, two from the lower concretionary zone and two from clays 10 feet below the upper concretionary zone, but a few feet apart, and on the same level in Quinn Draw basin. One of the latter, No. 12609, Plate I., Fig. 1, is uncrushed and entire, and agrees, within a few hundredths of an inch, with the dimensions of the egg of the Florida duck, *Anas fulvigula,* given by Farrington [49] (2.16 inches x 1.56 inches, or 55 mm. x 39.5 mm. as against 2.05 inches x 1.52 inches in the modern form). The shell has lost

[49] Farrington, O. C., "A Fossil Egg from South Dakota," Field Columbian Museum Publication No. 35, Geol. Series, Vol. I., No. 5, pp. 193–200, 1899.

its carbonate content, is black, rugose, and much pitted. The outer table of the shell is considerably eroded, perhaps by sand blast or solution, and at a few points where it is completely flaked off, reveals a deposit of chalcedony below. This egg is quite heavy, suggesting the presence of barite in the unexposed center. The other specimen from the same locality and level, No. 12613, also has a black, much pitted, shell, seemingly impregnated by chalcedony and encloses a mass of chalcedonized pseudomorphs arranged in radiating plates. It also is quite heavy. Both eggs from the lower concretionary zone have the shells badly crushed and eroded, and are too distorted to subject to measurements.

An extremely fine fossil egg, No. 12616 Princeton University Geological Museum, presented by Mr. J. G. Bump of Scenic, still retains some of the carbonate content of its shell, and although distorted by pressure is not sufficiently fractured to show the interior filling. Its dimensions (57.5 mm. x 45 mm. x 40 mm.) and general appearance recall the second of two eggs figured by Mr. Troxell.[50] The shell is rugose but the pittings are less conspicuous than in No. 12609. It is shown on the left in Plate I., Fig. 1, and is, supposedly, from the White River series, but the locality and horizon are unknown, as it was acquired by Mr. Bump from an Indian, who was said to have found it somewhere in the vicinity of White River.

Reptiles.

1. *Peltosaurus.*

Parts of the skull and jaws of a lizard from the lower concretionary zone in Corral Draw have been identified by Mr. C. W. Gilmore as *Peltosaurus granulosus* Cope.

2. *Stylemys.*

The contrasting numbers of these animals listed from the two concretionary zones is in keeping with the limited thickness and poverty of favorable exposures of the upper zone, which too often outcrops on cliff faces. They must have occurred by the thousand, for

[50] Troxell, E. L., "Oligocene Fossil Eggs," *Jour. Wash. Acad. Sci.*, Vol. 6, 1916, pp. 422–425.

hundreds of their carapaces, in all stages of demolition, were seen along the many miles of exposure of the lower concretionary zone collected over by us. They early attracted attention, and are referred to in disparaging terms in T. A. Culbertson's quaint diary [51] under date of May 14, 1850, as "ugly, dark red, unshapen masses—these my guide told me were petrified turtles, their shells being destroyed by the action of the sun, and they are crumbling to pieces. The ground in many places is thickly covered with small lumps, the broken remains of turtles which a few years ago were perfect." They are supposedly land forms and their mode of occurrence in the concretionary zone, discussed in my paper on the "Turtle-Oreodon Layer," is quite in keeping with the theory of origin of the latter there enunciated.

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[51] Fifth Ann. Rept. Smithsonian Institution, 1851, p. 93.

THE SONIC DEPTH FINDER.

By HARVEY C. HAYES, Ph.D.

(Read April 26, 1924.)

Previous Papers.

This paper is the third of a series dealing with developments that have resulted from research work in the field of submarine acoustics carried out by the U. S. Navy during the past five years. The first paper, entitled "Detection of Submarines,"[1] described a superior type of submarine sound detector called the MV Hydrophone which had proved effective during the war and predicted that this device would eventually prove to be a distinct aid and safeguard to navigation. The second paper, entitled "The U. S. Navy MV Type of Hydrophone as an Aid and Safeguard to Navigation,"[2] described the various ways in which the MV Hydrophone could serve the navigator, as determined on various ships of the Navy, and gave the results of some practical tests.

Present Paper.

The present paper may be regarded as an extension of the second, inasmuch as it has to do with developments that are of prime importance to the navigator and which were predicted in that paper. It is also closely related to the first paper since it deals with an installation of submarine sound apparatus for ships that includes the MV Hydrophone. In order to avoid repetition the author will, therefore, only describe very briefly those parts of the sonic depth-finding apparatus that have been fully described in the two previous papers.

General Description.

The sonic depth-finding apparatus comprises three distinct units, viz.: a submarine sound transmitter, a submarine sound receiver

[1] *Proceedings of the American Philosophical Society*, Vol. XIX, No. 1, 1920.

[2] *Proceedings of the American Philosophical Society*, Vol. LIX., No. 5, 1920.

and the so-called "Sonic Depth Finder," which is a connecting link between the transmitter and receiver and which serves to accurately measure the time interval between the sending of a submarine sound signal and the return of its echo from the sea-bottom or any other submarine sound-reflecting surface. This time interval, together with the velocity of sound in sea water, serves to determine the distance to the reflecting surface. And if the sound receiver is a type that can determine the direction of the echo, as is the MV Hydrophone, then the reflecting surface can be definitely located and its slope determined because the direction of the echo is perpendicular to the reflecting surface. Such an installation of sound apparatus can, therefore, furnish complete data for making a survey of the sea bottom. The three units comprising the installation will be described in turn.

The Submarine Sound Transmitter.

The submarine sound transmitter consists essentially of a rigid steel diaphragm about eighteen inches in diameter one side of which is in contact with the water and to the other side of which is attached electrical means for exerting alternate thrusts and pulls of sufficient magnitude to cause the diaphragm to vibrate against the highly incompressible water and of sufficient rapidity to give an audible sound. The driving force is usually produced by powerful electromagnets through the coils of which is passed an alternating current. The force so exerted on the diaphragm may reach several tons. Such a transmitter, which is commonly called a submarine sound oscillator, can be used for sending code signals if it is provided with a suitable transmitting key for making and breaking the A.C. circuit.

The pitch of the sound generated is dependent on and numerically equal to the frequency of the A.C. current. The coefficient of reflection of the sea bottom is doubtless a function of the pitch of the sound but very little is known of this function. As a result the pitch of 1100 which is used has been chosen from considerations other than the coefficient of reflection. This comparatively high pitch is employed for two reasons. First, it permits the use of a high-pass filter that greatly weakens the low-pitched local noises but allows the signals to pass. Secondly, high-pitched transmitters are better adapted for sending short signals in rapid succession. It

may be stated that signals having any pitch between 550 and 1100 have been found to reflect well from the sea-bottom.

The Submarine Sound Receiver.

The submarine sound receiver may be a very simple and inexpensive device provided it is not required to determine the direction of the sound. It will be shown later that good sounding data can be taken with such a receiver, but for securing data of the highest accuracy it becomes necessary to determine the direction of the echo. The MV Hydrophone, developed by the U. S. Navy during the war has proved to be superior to other types of direction finding sound receivers for use on ships for the reason that it is simple and rugged, permits of easy and rapid manipulation, responds with almost equal sensitivity to sound waves having a wide range of frequency, and through the use of several receptors intensifies the response to sound waves from any one direction while at the same time the response to sound from all other directions is weakened. As a result much of the interference from the numerous local noises such as are caused by propellers, auxiliary machinery and slapping of waves is eliminated, thus enabling the operator to hear comparatively weak signals and echoes which otherwise would not be audible.

The MV Hydrophone employs a double telephone head set, one receiver of which connects with one half of the submarine sound receptors and the other of which connects with the other half of these receptors. Direction of sound is determined through the operation of the binaural sense and is accomplished by so varying the time of energy transit from each submarine sound receptor to its respective telephone receiver that the responses from all of these receptors reach the two phones at the same time and in phase. Under such conditions the sound will appear to the listener to be binaurally centered, *i.e.*, it will give him the impression that the source is located neither toward his right nor his left. The time of electrical transit of the several receptor circuits is varied by means of an "electrical compensator" through which all the receptor circuits pass before reaching the telephones, and the amount of retardation that must be introduced into these circuits to bring their currents into consonance at the phones determines the direction of the sound.

The electrical compensator is so ingeniously designed that the variations of current in the several receptor circuits, caused by the action of sound waves actuating the receptors from any definite direction, can be brought into consonance at the telephones by turning a single hand-wheel. This hand-wheel operates a rotary scale that is calibrated to give the direction of the sound with respect to a straight line passing through the sound receptors.

Two lines of sound receptors are employed, one for receiving sounds off the port and one for the reception of sounds off the starboard side of the ship. They occupy symmetrical positions on opposite sides of the hull, are placed as far forward and as far below the water-line as conditions will permit, and are directed parallel with the ship's keel. Only half of the sound receptors are used at any one time and the compensator is provided with a multipole switch whereby all the receptors on either side of the ship or the forward or rear half of the receptors on both sides of the ship can be connected through the compensator. When either of the first two combinations is used the compensator gives the direction of the sound with respect to the ship's keel and when either of the last two combinations is employed the direction of the sound is given with respect to a line running athwart ship and perpendicular to the keel.

The Sonic Depth Finder.

The Sonic Depth Finder is an automatic key used to make and break the A.C. circuit of the transmitter. It is designed to close this circuit for an instant at the end of equal time intervals and contains means for continuously varying this interval through all values included between about ten seconds and one tenth of a second. It consists of a phonograph-like disk rotated at constant speed by means of a tuning-fork-speed-regulated motor. A small wheel having its axis parallel with a radius of the disk is driven by friction contact with the surface of the disk and carries means for moving it radially in and out along its axis. The axis of this wheel carries two cams, one having a single saw-tooth-shaped depression and the other carries ten such depressions uniformly spaced about its circumference. Each cam operates a pair of contact points, and by means of a two-way switch either pair can be connected in series with the coil of a

relay. The relay operates a pair of heavy contacts connected in series with the transmitter circuit.

In practice the disk is driven at the uniform rate of one revolution in ten seconds and the ratio of the diameter of the disk to the friction wheel is ten. Such an arrangement permits the speed of the friction wheel to be varied from one revolution in about ten seconds to one revolution per second, and by making use of the two cam wheels the time interval between oscillator signals can be given any value between ten seconds and one tenth of a second. It is obvious that this period can at all times be computed from the radius of the circle that the friction wheel scribes on the disk. This radius can be determined to within a very small fraction of an inch by means of a micrometer arrangement used for sliding the friction wheel along its axis.

In order that the operator may use the same pair of head phones for determining the direction of sounds picked up by the MV Hydrophone and also for making depth determinations, the telephone terminals of the compensator are connected by cable to the Sonic Depth Finder where, by means of a two-way multipole switch, both phones can be connected to the compensator or one phone can be connected to the compensator and the other to the secondary coil of a vario-coupler the primary of which is energized by an inductive take-off from the A.C. circuit of the sound transmitter. These two positions of the selector switch may be called the (*A*) and (*B*) position respectively. The (*A*) position connects the two phones directly to the compensator. The apparatus is then adjusted for determining the direction of any sound picked up by the sound receptors and among others the echoes of the oscillator signals. The (*B*) position connects one phone inductively to the transmitter circuit and the other to the compensator. Under such conditions the operator hears each oscillator signal in the inductively connected phone at the instant it is transmitted and he hears the echo of each signal in the other phone which connects with the MV Hydrophone receptors. The vario-coupler serves to bring the signals in the two phones to approximately the same intensity.

Principle of Operation.

Numerous devices have been developed to measure the time interval between a submarine signal and the return of its echo but none have proved to be practical. This failure has been largely due to the fact that they attempt to measure the time interval in question directly. The success of the Sonic Depth Finder lies in the fact that it does not attempt to measure this time interval directly but measures the time interval between signals and adjusts this interval so that it is a simple known function of the interval between signal and echo. This is accomplished as follows:

Let (t) represent the time interval required for the sound to travel from the transmitter to the reflecting surface and back again to the hydrophone receivers and let (p) represent the period between successive signals. If the operator adjusts the value of (p) to equality with (t), then at the instant he hears a signal transmitted through the inductively connected phone he will also hear the echo of the previous signal in the other phone. This adjustment can be made by moving the friction wheel to a proper position along the radius of the constant speed disk. Suppose now he increases the frequency of the signals until (p) is equal to one half of (t). Then he will still hear the signals and echoes simultaneously in the two phones but each signal will now arrive coincident with the echo of the second previous signal. Finally, if the period between signals is made equal to one $(N\text{th})$ of (t), the operator will still hear the signals and echoes coincident in the two phones but each signal is then transmitted simultaneously with the arrival of the echo of the $(N\text{th})$ previous signal.

It will, therefore, be seen that the operator will hear the signals and echoes simultaneously in the two phones whenever he adjusts the position of the friction wheel so that (t), the time interval between a signal and the return of its echo, is equal to some whole number (N) times (p), the period between signals. Conversely, if the operator wishes to take a depth sounding and adjusts the Sonic Depth Finder so that he hears the echoes and signals simultaneously in the two phones, he knows that

$$t = N \cdot p,$$

where (p) is the period between signals, (N) is a whole number and (t) is the interval of sound transit. The value of (p) can be determined from the micrometer scale of the mechanism used to move the friction wheel and it only becomes necessary to determine (N) to completely determine (t).

The factor (N), which is always a whole number, can be determined in various ways. If we remember that (N) represents the number of signals that intervene between the one heard in the inductively connected phone and the one whose echo is heard at the same instant in the other phone, it becomes clear that (N) may be defined as the number of signals in transit between the transmitter and the hydrophone at any time. Therefore, (N) can be determined by counting the number of echoes heard after the transmitter is stopped or by counting the number of signals heard before an echo is heard when the transmitter is started. The value of (N) can always be definitely made unity by starting with a very large value of (p) and then reducing the value until coincidence between signals and echoes is reached. A third method of determining (N) will be described later.

The operation of the Sonic Depth Finder depends upon bringing to coincidence the arrival of two similar sounds in two telephone receivers, one on each ear of the operator respectively. Little or no training is required to judge such a condition of simultaneity to within a very small fraction of a second and as a result this method of determining the time interval between a signal and its echo gives surprisingly accurate results.

Development of the Sounding Equation.

Let (S) equal the distance a sound signal travels in passing from the transmitter to the receiver by way of reflection from some submarine surface and let (t) equal the time of transit in seconds. Also let (V) equal the velocity of sound in sea water.

(1) Then $S = V \cdot t$

(2) and $S = 2H$, where (H) is the perpendicular distance to the reflecting surface.

(3) Wherefore $H = \frac{1}{2} V \cdot t$.

We have seen that

(4) $t = N \cdot p$,

where (N) and (p) represent respectively the number of signals in transit and time interval between signals when the apparatus is so adjusted that the signals in one phone are simultaneous with the echoes in the other. Substituting this value for (t) in equation (3) gives:

(5) $H = \frac{1}{2} V \cdot N \cdot p.$

While this formula is exact, it is not workable until the factors (N) and (p) are determined in terms of the dimensions of the Sonic Depth Finder and its scale reading. Let the radius of the circle which the friction wheel scribes on the constant speed disk be (R) and the radius of the friction wheel itself be (r). Let (P) be the time required for the disk to make one revolution and let (C) represent the number of teeth on the cam used to operate the contact points.

Order Number 20353.

Since there is no slip between the disk and friction wheel, it follows that:

(6) $$\frac{2\pi R}{P} = \frac{2\pi r}{p}.$$

(7) Wherefore

$$p = \frac{r \cdot P}{R}$$

when a single-toothed cam is used and in general becomes

$$p = \frac{P \cdot r}{C \cdot R},$$

when the cam-wheel carries (C) teeth equally spaced about its circumference.

Substituting this value for (p) in equation (5) gives:

(8) $$H = \frac{V \cdot N \cdot P \cdot r}{2C \cdot R}.$$

Suppose now that the Sonic Depth Finder has been adjusted carefully for coincidence between signals and echoes. Call this scale reading (R_1). Then increase the frequency of the signals until signals and echoes are again coincident and call the scale read-

ing (R_2). Now since the value of (H) is the same in both cases it follows that:

$$(9) \qquad \frac{V \cdot N \cdot P \cdot r}{2C \cdot R_1} = \frac{V \cdot (N + 1) \cdot P \cdot r}{2C \cdot R_2},$$

(10) wherefore

$$N = \frac{R_1}{R_2 - R_1}.$$

Substituting this value for (N) in equation (8) we have for the general sounding equation:

$$(11) \qquad H = \frac{V \cdot P \cdot r}{2 \cdot C \cdot (R_2 - R_1)},$$

where (H) is the perpendicular distance to the reflecting surface, (V) is the velocity of sound in sea water, (P) is the constant period of revolution of the disk, (r) is the radius of the friction wheel, (C) is the number of teeth on the cam, and $(R_2–R_1)$ is the distance the friction wheel must be moved outward in passing from one condition of coincidence between signals and echoes to the next in order.

It is to be noted that $(R_2–R_1)$ holds for all values of (R) and therefore for the condition (R_1) equals zero. Hence $(R_2–R_1)$ is the radius of the circle scribed on the disk when the apparatus is adjusted for coincidence of signals and echoes and (N) is equal to unity.

The Sonic Depth Finder developed and used by the U. S. Navy has the following values for the constants of the sounding equation (11):

$$P = 10 \text{ seconds},$$
$$r = 1,$$
$$C = 1 \text{ or } 10.$$

The average value of 4,800 feet per second or 800 fathoms per second is taken for (V), the velocity of sound in sea water. Both (R_2) and (R_1) are measured in terms of (r) as a unit. This is accomplished by making the pitch of the micrometer thread equal to (r) and this permits of making (r) equal to unity in the sounding equation. Inserting these values in equation (11) gives for the practical sounding equation:

$$H \text{ (feet)} = \frac{24{,}000}{C(R_2 - R_1)},$$

where (C) may be (1) or (10), depending upon which cam is employed in operating the transmitter.

$$H \text{ (fathoms)} = \frac{4{,}000}{C(R_2 - R_1)}.$$

Accuracy.

The accuracy with which the range (H) to a submarine reflecting surface can be determined by the Sonic Depth Finder depends finally upon the accuracy of each of the several factors on the right-hand side of the sounding equation:

$$H = \frac{V \cdot P \cdot r}{2 \cdot C \cdot (R_2 - R_1)}.$$

The factor (V), which represents the velocity of sound in sea water, varies with the temperature, salinity and pressure. The temperature coefficient at a depth of about 180 feet is approximately 2.8 feet per degree centigrade and it is probable that the variation in this factor caused by pressure or salinity may be regarded as a second order effect and neglected. No reliable data is extant relative to the effect of pressure and salinity on the velocity (V), but from theoretical considerations we are assured that both effects are small.

The temperature gradient of sea water along a vertical direction has been shown to be much the same over most of the ocean areas below depths of a few hundred fathoms and it is a favorable circumstance that the change in velocity of sound caused by increasing pressure, as greater depths are approached, is nearly compensated for by the change produced by the decrease in temperature. As a result, there is reason to believe that the factor (V) does not vary more than a fraction of a per cent. over most ocean areas for depths below from 300 to 500 fathoms and it has been found that 4,800 feet per second is a good average value for the factor (V) for use in taking deep-sea soundings. For depths within from 300 to 500 fathoms a temperature correction may be made to this value if other factors warrant such accuracy.

The factor (P), the period of revolution of the disk, is readily maintained constant and accurate to within a tenth of a per cent. and the radius (r) of the friction-wheel can be made with almost any required degree of accuracy. No error is introduced by the factor (C) as its value is definitely one or ten, depending upon which cam is used. The accuracy of (R_1) or (R_2) depends upon the accuracy with which the operator can judge of the simultaneity of two signals. The (R) factors can be determined to within about two thousandths of an inch, and since their minimum value is about a half inch the error in this factor cannot amount to more than one or two tenths of a per cent.

In practice it has been found that with the sounding ship at rest over a comparatively level sea bottom repeated settings of the Sonic Depth Finder agreed with one another to within from one to two fathoms. The value of (H), so determined, may be in error several fathoms due to uncertainty in the value of (V), but *differences* in (H) are given with but small error. And it is such information that is of importance in making a survey of the sea floors.

Depth Sounding.

Thus far we have only spoken of the method and apparatus for determining (H), the distance from the ship to the submarine reflecting surface along a perpendicular to that surface passing through the ship. The depth (D) is the distance from the surface to the sea bottom along a line perpendicular to the surface of the water. It is obvious that (D) and (H) are equal only when the sea floor is horizontal. If the angle of dip (δ), which the sound path (H) makes with the surface, is known, then:

$$D = H \cdot \text{sine } \delta,$$

where (D) is the depth along a vertical line passing through the point of reflection and *not* the depth underneath the ship, which may be called (D^1). This depth cannot be definitely determined with the Sonic Depth Finder. However, if it is assumed that the slope of the sea bottom is uniform over an area including a point directly beneath the ship and the point of reflection, then,

$$D^1 = H, \text{ sine } \delta.$$

These statements will be more readily understood by considering Fig. 1, wherein the lines $(S–S)$ and $(B–B)$ represent the surface and sea floor respectively and points (1), (2), (3) and (4) represent the position of the ship, the point of reflection, the point vertically above the point of reflection and the point vertically beneath the ship respectively. The distances (D), (H) and (D^1) have been defined and the angles (δ) and (β) are the dip of the line (H) below the surface $(S–S)$ and the slope of the sea floor respectively. It is to be noted that (β) can be determined from (δ) through the relation

$$\delta + \beta = \frac{\pi}{2} \cdot$$

As stated, (H) is not the depth. The depth (D) over point (2) can be definitely determined if the angle (δ) is known, since

$$D = H \text{ sine } \delta,$$

and if the slope between points (2) and (4) is constant, then (D^1) can be determined through the relation

$$D^1 = H \text{ sine } \delta.$$

The probable truth of the assumption that (β) remains constant over an area including points (2) and (4) is dependent on the distance between these two points. This distance is proportional to (H) and to $(\cos \delta)$. It is believed that no great area of the sea floor has a slope as great as 30 degrees and, therefore, the minimum value of (δ) will not be less than 60 degrees. There is also reason for believing that no great area of the oceans has a depth greater than 6,000 fathoms or six miles. Therefore, the greatest probable distance between points (2) and (4) will approximate three miles and in general be very much less than this figure. Of all the submarine surveys that have thus far been made with the Sonic Depth Finder, no slope greater than 15 degrees has been found, and under such conditions the distance between points (2) and (4) where the depth is 6,000 fathoms would be about a mile and a half. At 3,000 fathoms, which is beyond the average ocean depth, this distance reduces to about three fourths of a mile. It is believed that the sea bottom is

much more regular than are the areas above sea level for the reason that it has been shielded from erosion. The slopes have resulted alone from warping of the earth's crust and as a result abrupt changes in the value of (β) should not in general be expected. It would therefore appear that the assumption in question may be expected to approach the truth even under the most unfavorable conditions and it seems certain that considerable reliance can be placed on the formula

$$D^1 = H \text{ sine } \delta$$

over depths and slopes such as to make the distance between points (2) and (4) within a mile. It must, however, be remembered that the accuracy of this formula depends upon an assumption that may possibly be far from the truth.

But why worry about (D^1), the depth of water directly underneath the ship, when the depth (D) at some point (3) at least within three miles of the ship can be accurately determined and especially when it is remembered that in general this point will be much less distant than 3 miles? For surveying purposes, at least, such a point on the chart furnishes as valuable information as would one at the exact location of the ship when the sounding was taken. In this connection, it is of interest to see what it may mean if we forget about (δ) and assume that (H) represents the depth. It can be shown that for all values of (β) less than 30 degrees, (H) is practically equal to one half the sum of (D^1) and (D). Therefore (H) may be regarded as the depth at a point half way between (1) and (3) and we begin to question whether it is worth while to determine the dip (δ) and make the resulting corrections which give the true depth (D).

This question can be definitely answered. Somewhere over the comparatively small area that includes points (2) and (4) the true depth is represented by (H) and the chances are that this point is about midway between points (2) and (4) or half way between points (1) and (3). The factor (H) will therefore represent the true depth at all times at a point within a mile and a half of the ship and usually at a point much nearer. The answer, therefore, is as follows: Unless a ship has some means of locating its position to

within a mile and a half nothing is to be gained by charting the correct sounding (D) in place of its approximate value (H). And since navigation by means of solar checks cannot give the ship's location to within this distance it follows that over most of the ocean areas the depth can be represented on the chart as well by (H) as by (D) or (D^1).

The next question that arises is: Why should an expensive receiver like the MV Hydrophone be used as a part of the depth-sounding installation when its ability to determine the direction of the echoes cannot be utilized because of uncertainty in the location of the ship. It has already been explained that the focussing ability of the MV Hydrophone enables it to detect faint signals that otherwise would be drowned out by the numerous comparatively intense local sounds. This fact, together with its ability to safeguard the ship in the various ways described in the second paper of this series, is sufficient argument for its use, even though it increases the cost of the installation. But for submarine surveying, and also for purposes of navigation, the direction of the echo is valuable for the reason that although a ship does not know its position on the chart to within about two miles, it does know the progress made along its course from hour to hour with sufficient precision to warrant all possible accuracy in the sounding data used for preparing or identifying a profile of the sea bottom along the route covered. By taking into consideration the slope of the sea bottom such profiles can be prepared with considerable accuracy and for scientific purposes the profile itself is more valuable than is its exact location on the chart. Moreover, such profiles are most valuable when they define a section perpendicular to the sea bottom and a determination of the direction of the echoes enables the navigator to direct his course so as to give such profiles.

It has been shown that the depth (D) can be accurately determined if the angle (δ) is known and it has been stated that the MV Hydrophone determines the direction of the echo with respect to the ship's keel and with respect to a direction across ship and perpendicular to the keel. So far as the angle (ϕ), which gives the direction with respect to the keel, is concerned the echo may proceed along any element of a conical surface having the submarine sound re-

ceptors at the apex and making an angle (ϕ) with the keel. Similarly, the angle (θ) which the echo makes with a direction athwart ship only limits its direction to an element of the surface of a cone making an angle (θ) with a direction athwart ship and having its apex at the sound receptors. The intersection of these two conical surfaces having the same vertex does, however, definitely determine the direction of the echo. Neither (ϕ) nor (θ), in general, are equal to (δ), the angle of dip, but they are definitely and simply related to this angle as can be understood by considering Fig. 2, wherein (O) represents the position of the ship on the surface defined by the intersecting lines (X–X) and (Y–Y). The ship's keel is coincident with (X–X), and (Y–Y) represents the direction athwart ship. The sound reflects from (S) and proceeds to the ship along path (H). The MV Hydrophone determines the angles (ϕ) and (θ), whereas the angles (α) and (δ), the azimuth and dip bearings, are desired. The relation between these two sets of angles obviously is:

(1) $\cos\phi = \cos\delta \cdot \cos\alpha$,

(2) $\cos\theta = \cos\delta \,\text{sine}\, \alpha$,

wherefore,

(3) $\cos\delta = [\cos^2\phi + \cos^2\theta]_{\frac{1}{2}}$,

(4) $\tan\alpha = \cos\theta / \cos\phi$.

The Sonic Depth Finder determines (H) and the MV Hydrophone furnishes data for determining (α) and (δ). The actual depth at (S) is given by the equation

$$D = H \,\text{sine}\, \delta,$$

and the vertical through point (S) intercepts the surface at a point out from the ship a distance (R) where

$$R = H \cdot \cos\delta,$$

and the line (O–R) makes an angle (α) with the ship's keel.

In making profile surveys it is desirable that (α) should be zero. The navigator can readily accomplish this by making his course such that (θ) is equal to ($\pi/2$). It is to be noticed that (ϕ) then is equal to (δ), the angle of dip, and the computations are very much simplified.

Comparison of Sounding Data.

The only condition under which sounding data taken by the deep-sea sounding machine and the Sonic Depth Finder can be compared is over regions where the angle (δ) is equal to ($\pi/2$), or in other words where the sea bottom is horizontal. Referring again to Fig. 1, (D^1) may perhaps be supposed to represent the depth as determined by the sounding machine (though it is quite possible that the lead may strike bottom at a point some considerable distance from that vertically beneath the ship), and for practical purposes we have seen that the depth may be taken as (H), which we should expect to be less than (D^1). The difference between soundings taken by the two methods may be represented by the difference between (D^1) and (H) and this value divided by (H) gives the variation in per cent. between the two methods, or

$$\text{Present Variation} = \frac{D^1 - H}{H} = \frac{1 - \text{sine } \delta}{\text{sine } \delta}.$$

The following table gives this percentage variation for various values of (δ) and also the actual variation for a depth of 1,000 fathoms.

Angle of Dip, (δ).	Percentage Variation, $(D^1 - H)/H$	Actual Variation, $(D^1 - H)$.
90	000	00
85	.004	4.0
80	015	15.0
75	035	35 0
70	064	64 0
65	.104	104 0
60	.153	153 0

Remembering that (β), the slope of the sea bottom, is the complement of (δ), it will be seen that the percentage variation between the sounding data given by the sounding machine and the Sonic Depth Finder averages about .15 of a per cent. per degree of slope and that the actual difference in the data amounts to about 1.5 fathoms per degree of slope for every 1,000 fathoms depth.

Practical Results.

The first practical design of the Sonic Depth-finding apparatus was installed on the U.S.S. *Ohio* and successfully tested in February, 1922, during a cruise from New York along a course running southeast from the Ambrose Light Ship to the latitude of Cape Henry and thence west into Chesapeake Bay. The greatest depth encountered was about 1,700 fathoms. The following June this apparatus was transferred to the destroyer U.S.S. *Stewart* just previous to her departure for Manila by way of Gibraltar and the Suez Canal. Throughout the run to Manila soundings were taken at least every twenty minutes and at times as often as every minute. The greatest depth encountered was about 3,200 fathoms. This ship is still based on Manila and during the past two years has collected a large amount of sounding data. Her reports state that almost no troubles have been experienced in connection with the sound installation. The *Stewart* will probably return this year by way of the Pacific, thereby completing a line of soundings around the earth.

Following the success of the *Stewart*, the destroyers *Hull* and *Corey* were equipped with the depth-sounding apparatus and directly proceeded to make a survey of about 35,000 square miles of the sea floor off the coast of California. This survey, which extended from the 100-fathom curve out to a depth of 2,000 fathoms was completed at the rate of about 1,000 square miles per day. These ships reported that the acoustical sounding apparatus gave no trouble and that it was rugged and able to withstand the adverse conditions often met with on sea-going vessels.

Since that time several other ships of the Navy have been equipped as has also the Coast and Geodetic Survey Ship *Guide*. These ships are taking soundings during every cruise with the result that the Hydrographic Office is receiving an enormous amount of reliable deep-sea sounding data which has already served to clear up several doubtful regions on the navigational charts and which is rapidly leading to an accurate charting of the depth along the main traffic routes. These data have disclosed discrepancies of hundreds and even thousands of fathoms between the actual depth and the old charted values and the discovery of submarine mountain peaks or ridges rising several thousand feet above the sea floor is not un-

usual. These landmarks will doubtless prove to be valuable for determining the location and progress of merchant ships that are provided with apparatus for taking deep-sea soundings.

The scientific value of the data that can now be collected with the sonic depth-finding apparatus can scarcely be overestimated. Such data will doubtless furnish valuable information concerning the movements of the earth's crust for the reason that the contour of the sea floor, which has been protected from the processes of erosion, is the direct result of such movements and also for the reason that the most unstable regions of the earth's surface, where the movements are so great and so violent as to affect areas far beyond their origin, form a part of the sea bottom. It may be expected that a careful study of the form and movement of the sea floor brought about by the earth-warping forces will lead to a better understanding of these forces.

Conclusion.

Research work dealing with submarine acoustics which the U. S. Navy has carried out during the past five years has led to the development of methods and apparatus capable of aiding and safeguarding navigation by enabling the navigator to locate sound beacons to a range of several miles and to readily determine the depth of water beneath his ship at all times. By means of this same apparatus it is possible now for the first time in history to accurately survey the ocean floor and a large amount of data pertaining to such a survey is being collected by the various ships of our Navy. While it is impossible at this time to estimate the importance of such a work it seems certain that any undertaking that adds to our meager knowledge of the great ocean areas must eventually prove to be a benefit to humanity.

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SEX IN THE RIGHT AND LEFT SIDES OF THE BIRD'S BODY.

By OSCAR RIDDLE.

(*Read April 26, 1924.*)

Much recent work indicates that sex in higher animals, though normally determined on a very definite and well-known genetic plan, is a very plastic, modifiable and even a reversible character. Other quite recent studies make it nearly certain that many of the "hermaphrodite" vertebrate animals described during the past few decades were not founded upon a genetic basis—as has been supposed by most biologists—but were really stages of sex-reversal which are to be described in physiological terms. Still other data on sexuality are coming to hand for which also it is fairly clear that physiology rather than genetics must supply the explanation. It is, apparently, a case of this kind with which these remarks are concerned.

If other data did not already indicate the plasticity of sex it would perhaps appear incredible that the two points at which the germ glands arise and develop on the right and left sides of the bird's body are points unequally favorable for the development of male and female sex glands. Even to some of those who regard the plastic nature of sex as fully demonstrated the suggestion that this positional relationship has a real sex significance may perhaps at first seem a rather astounding suggestion. It is not assumed that all biologists are ready to agree to the reversible nature of sex; and we may also grant that possibly the conception to which we now invite attention is wholly wrong. It is here our purpose, however, to mention four kinds of facts which seem to suggest the reality of the proposition stated above. Whether this conception is right or entirely wrong it will certainly require attention until several lines of fact now at hand are otherwise adequately explained. It may be said at once that we are not necessarily concerned with the right and left side of the bird's body as a whole, as our title might suggest,

but merely with the points on the right and left sides at which the germ glands arise and develop.

The first kind of evidence to be presented is based on two series of facts: (1) It has long been known that in female birds the right ovary, though it forms in the embryo, begins to atrophy shortly before hatching and no right ovary is normally present in the adult bird. This is apparently true of all species of birds. Lilienfeld, Nagel and Pick have all made short comment on the bearing of this different expression of femaleness on the right and left sides of the bird's body. Apart from any theory it is a fact that ovarian tissue develops to a far greater extent on the left side in all birds. This fact standing alone would appear to have a sexual significance; when considered in connection with the facts next to be mentioned concerning testis size on the two sides of the body such a significance becomes wholly probable. (2) The other half of this situation has been learned chiefly in our own studies. In two earlier reports we have shown in part, and can now establish the fact satisfactorily on a number of species of pigeons, that in most cases (enough to establish a definite rule) the right testis of pigeons is larger than the left.

In connection with the above observation we are learning that disease, confinement of the animals, and hybridity are all agencies which markedly affect the better development of the testis on the right side. They all lessen the proportion of pairs of testes in which the right is the larger (Table I.). Incidentally it may be noted that it is now becoming clear that each of these three factors has a definite effect either upon sex expression (including change in gonad size) or upon the sex ratio; we also think there is good evidence that they may and sometimes do profoundly modify sex itself. When these modifying factors are applied to data earlier obtained by others (for relatively few species in other orders of birds), and after personally securing some fragmentary new data for some of these forms, we obtain at least some evidence for a wider application of this observation to other birds than pigeons.

The insufficient data as published, however, are in opposition to the situation found in pigeons. Etzold, Gadow and others report larger left testes in birds in general. It now seems probable that failure to consider effects of confinement, disease and hybridity,

together with unsuitable material obtained at times other than the height of the bird's breeding season, are responsible for previous failure to find in at least some other birds a condition similar to that found by us in the pigeons. In individuals of several wild bird species taken at one or another stage of gonad regression we too have found that the right testis is quite generally smaller than the left; but it is possible that this may often indicate merely that the right has suffered a more pronounced reduction than has the left. The right testis of pigeons also suffers greater reduction than the

TABLE I.

Data Showing Prevalence of Larger Right Testes in Various Groups of Pigeons. Also Effects of Age (Confinement) and Disease.

Classifications.		Age and Health.	Ratio $L- : L+$.	Number of Pairs.
Kind of Pigeon.	Species.			
Free breeders in confinement	St. risoria	Adult healthy........	12.0 : 1	36– 3
		Adult diseased	6.4 : 1	58– 9
	Com. pigeons	Adult healthy......	4.5 : 1	165–37
		Adult diseased......	6.2 : 1	167–27
		Adolesc. healthy ...	4.3 : 1	26– 6
		Adolesc. diseased ..	3.8 : 1	19– 5
		Embryos alive......	34.0 : 1	102– 3
		Embryos dead......	25.2 : 1	277–11
Poor breeders in captivity	Several wild species	Adult healthy	0.6 : 1	5– 8
		Adult diseased.......	0.95 : 1	19–20
		Juv. healthy......	6.0 : 0	6– 0
		Juv. diseased. .	2.0 : 1	22–11
Fertile hybrids breeding well in captivity	All specific hybrids	Adult healthy......	8.0 : 1	223–28
		Adult diseased......	6.8 : 1	232–34
		Embryos alive......	5.6 : 1	333–63
		Embryos dead.......	4.8 : 1	140–29

left when put under conditions adverse to testis growth (confinement of wild species; disease). Data on this point will be found under "wild species of pigeons" in Table I. For most bird species other than pigeons the relative size of the two testes at the height of their development is now unknown and the point requires a more thorough and circumspect investigation in birds in general than has yet been made. It has earlier been shown by us that in pigeons the shape of the left testis is more nearly that of the (elongate) ovary.

We have now observed that this more elongate shape is also found in the left testis of many other wild birds (starling, English sparrow, etc.), although in these species the left testis is larger during at least most periods of the year.

It can be said therefore that the left side of the body is more favorable for ovarian development in all birds; that the right side is more favorable for the growth of testicular tissue in at least eleven species of pigeons belonging to two different families of this order, and that it is also found to a now unknown extent in other orders of birds; that conditions adverse to testis growth usually, in pigeons certainly, produce either greater reduction or more frequent reduction in the right testis; and finally, that the shape or form of the left testis more nearly resembles the ovary than does the right testis.

A second line of evidence is supplied by data which we have obtained on the occasional persistence of the right ovary in pigeons. Throughout several years of study it has been observed that this persistence is definitely more marked and more frequent in embryos hatched from ova in which the metabolic rate had been measurably reduced. These are the same conditions which we have found to increase the proportion of female offspring. In a considerable number of these cases the right ovary continues to undergo a slow development instead of degenerating and disappearing; while in a few such cases the right ovary has not only persisted but has fully functioned and liberated mature ova.

This second line of evidence also has a second side to it. In our earlier work it was found that when hybrids are formed from species more and more distantly related (even species from different zoölogical families are partially fertile in a first cross of pigeons) the resulting hybrids show higher proportions of males corresponding to the greater width of the cross. At the present time we are able to show that parallel with this change in the sex ratio there is also a change in the proportion of right gonads which are larger than their associated lefts. In other words, from male individuals produced in those crosses in which we are presumably changing most female-producing eggs into male embryos we obtain the highest proportion of males whose glands resemble female gonads in their size relations

—namely in having larger left gonads. This is what would be expected if there is an initial element of "femininity" in males developed from female-producing eggs.

TABLE II.

SUMMARY ON SIZE RELATIONSHIP OF RIGHT AND LEFT TESTES AS AFFECTED BY HYBRIDITY (HEALTHY ADULTS).

Kind of Bird.	Ratio $L- : L+$.	Weight Relations and No. in Each Class.					
		No.	Weights (mgm.).		No.	Weights (mgm.).	
			Right.	Left.		Right.	Left.
St. risoria	*12.0 : 1*	36	624	494	3	410	425
Streptopelia hybrs.	*9.5 : 1*	133	571	466	14	465	519
All specific hybrs	*6.9 : 1*	90	584	492	14	600	654
Complex hybrs.	*3.4 : 1*	131	555	436	39	400	698
Generic hybrs.	*3.1 : 1*	58	476	394	19	412	486
Sub-family hybrs.	*2.0 : 1*	2	392	297	1	253	268
Family hybrs.	*0.4 : 1*	3	579	486	8	495	816

Table II. shows the frequency of this reversal of the normal size relations of the right and left testis in six groups of healthy adult hybrids. These hybrids are listed in order of the width of cross involved—the widest or most distinct cross being placed at the bottom of the table. It will be observed that there is a steady reduction of the proportion of testis pairs in which the left testis is smaller—from 9.5 : 1 with least crossing to 0.4 : 1 in the widest (family) cross. These data would seem to show conclusively that the dissimilarity, or width of the cross, is very definitely related to the observed change in the normal size relations of the right and left testis in the offspring. This fact can now be joined to the further and earlier demonstration (the studies of several observers of other bird hybrids, and the studies of Whitman and the writer on pigeon hybrids specifically) that the sex ratio of hybrid birds is modified in parallel with this change in size relationship of the two testes. If the last-named fact and the data of Table II. constituted the sole evidence on the present subject they would seem sufficient to raise a presumption that the points at which the two testes develop are not equally favorable for the development of testicular tissue.

A third line of evidence can now be obtained from data gathered during the last three decades on hermaphrodite birds. We have examined all of the recorded cases of hermaphroditism in birds and find in every described case in which gonadal tissue was present on both the right and left sides of the body that the male or testicular tissue was found on the right side of the body and ovarian tissue only, or ovarian tissue in connection with testicular tissue, was present on the left side of the body. In all of these sixteen [1] carefully examined cases it is evident that some influence so acted as to produce male tissue on the right side of the body and ovarian tissue on the left. In three of these cases (those of Weber, Lorenz and Poll) it is quite probable that one or another form of genetic explanation is the true one, since in each case male somatic characters were also found on the right side, and female somatic characters were present on the left side of the body.[2] On the other hand most, possibly all, of the remaining thirteen cases probably receive their explanation on physiological grounds. It seems inadmissible that in all of sixteen consecutively observed cases the blastomere corresponding to the right side of the embryo should have received a male-determining and the left a female-determining element from the first cell division; or that a similar genetic foundation should have been so discriminatingly supplied from any other source. Still more to the point is the fact that in the two most adequately observed of these cases (those of Crew and of Riddle) this possibility has now been practically excluded.

The two last-named cases throw much light on the real nature of the hermaphroditism most frequently encountered in birds. Crew was able to show that in one of the fowls which he examined the body had passed through an initial female stage into a stage at which a testis developed on the right side and in which testicular tissue was able gradually to replace the ovarian tissue of the left side as this was gradually destroyed by a tuberculous ovarian tumor. Before

[1] Such cases from Brandt, Weber, Lorenz, Shattock and Seligmann, Poll, Pearl and Curtis, Boring and Pearl (3); Hartman and Hamilton, Pezard and Caradroit, Macklin, Crew and Fell (3) and Riddle are included.

[2] The gonads of even these cases offer some difficulties for purely genetic explanation The "primordial germ cells" (see later) *circulate* before lodgment in right or left gonad.

its death Crew was able to show that this bird, earlier a mother of chickens, was producing sperm. Later, while used as a male parent, it also became the father of chickens. At autopsy ovarian tissue was essentially absent and the bird had typical testes on both the right and left sides of the body. A complete sex-reversal in the adult could here be demonstrated. Five years earlier the writer had observed, and has since reported, a case in a blond ring dove in which the gonad history quite duplicated the conditions found by Crew in the fowl—though in our case the transformed bird was not tested as a male parent. The data obtained on both these birds make it practically certain that there was a relatively long period in each bird's life when it possessed a testis on the right side and an ovary or an ovotestis on the left side. These are precisely the conditions in which the remaining eleven hermaphrodite birds were found. These observations therefore make it probable that many recorded bird hermaphrodites arose from the circumstance that testicular tissue arises more readily on the right side of the body than on the left.

The one-sided nature of the distribution of testis and ovarian tissue in bird hermaphrodites naturally raises the question whether a similar condition is found among the hermaphrodites of other classes of vertebrates. This topic is reserved for extended treatment elsewhere in connection with our data on unusual gonad conditions resulting from hybridity, but it may be noted here that in the lowest mammals (monotremes) the left ovary is known to be larger than the right. In a greater proportion of the accredited cases (7 : 1) of human hermaphrodites most testicular tissue was found on the right and most ovarian tissue on the left sides of the body. Apparently a somewhat similar condition and proportion (6 : 3) has been recorded in swine, though more meagre data indicate the opposite condition in some other mammals (deer, cat). Arai states, on the basis of adequate data for normal (not hermaphrodite) individuals in the rat, that as a rule the left ovary is larger. Among hermaphrodite amphibia of the genus *Rana* more testis was found in the right and more ovary in the left gonad in 14 : 5 cases; in genera other than *Rana* a similar difference (4 : 4) has not been found. Among Elasmobranchs some groups have only the right ovary functional and in others only the left ovary is functional.

These variations within each of the vertebrate classes would seem to call for special anatomic-physiologic studies; studies on differences in blood supply of the two gonads in genera differing in the above respect, and a much more definite knowledge of the relative size of the two testes in these groups, being particularly desirable.

A fourth line of evidence on the present topic is found in the following single observation published now four months ago. Benoit removed the single left ovary from two young chicks when four days and twenty-six days old. Several months later these birds were killed and each was found to have developed testicular tissue on the right side of the body. In these cases there can be no presumption of a cytological foundation for the development of testicular tissue. In physiological terms, however, it can be said that the removal of the restraining influence of the growing left ovary permitted the growth of testicular tissue at the point—on the right side—where testis develops best and where ovarian tissue is most retarded (the common fowl is a form in which there is evidence that the fully developed right testis is larger than the left).

The four very different kinds of evidence thus briefly mentioned, together with still other considerations not practicable to include here, all become harmonized in their suggestion that the easier and more frequent appearance (and sometimes or often a greater amount) of testicular tissue on the right side of the bird's body is a matter which rests upon a physiological rather than a genetical basis; that testis develops better than ovary on the right side; and that ovarian development is more favored than testicular development on the left side of the bird's body. It is thought that no known fact is opposed to any part of this suggestion.

Something may be said concerning a possible explanation of this positional differential. Though a definite explanation can not now be given, this discussion requires the mention of three points which to us seem a probable basis of the observed difference. First, Firket and Swift have shown that in the early embryo of the bird more "primordial germ cells" constantly collect at the future site of the left gonad. These cells constitute the "Keimbahn" in birds and play a leading part in the entire history of the gonad; at an early stage these cells enter the embryonic blood vessels and for some

unknown reason two to five times as many of them find lodgment at the seat of the left gonad as at the right. The conditions which determine this unequal distribution of primordial germ cells is probably the first condition causally related to the asymmetrical gonad growth which has been discussed in this paper. After an unequal accumulation of these cells has occurred on the two sides of the embryo it may be presumed that this inequality then itself enters into the establishment of further conditions unequally favorable to gonad growth on the right and left sides.

A second point which may find application here has already been suggested by Benoit. Benoit believes that the results of his castration experiments cited above really indicate a more ready formation of testis tissue on the right side than on the left, and in explanation he cites Firket's conclusion that only the first proliferation of sex cords occurs in the right ovary of the fowl. Since only the one set of cords form in the male gonad (testis) while two form in the functional female gonad (left ovary), it is suggested that this circumstance makes easier the origin of testicular tissue from the right than from the left ovary. We agree with Benoit that a fact of real importance is probably involved in the difference just described. The facts as found by Firket for the fowl cannot, however, apply fully to our numerous cases of pigeons with numerous oöcytes in their (persistent) right ovaries; for, in these ovaries a second proliferation of sex cords has obviously occurred but such right ovaries nevertheless remain always much smaller and much less active than their associated left ovaries. We should prefer to regard an ovary in which only one instead of two proliferations of sex cords has occurred as simply less completely differentiated ovarian tissue. On the one hand, its lesser differentiation is in some way associated with its position; on the other, it is associated with greater ease of transformation into differentiated tissue of another kind, namely testis.

A third consideration touches the possible existence and effects of a dissimilar or unequal blood supply to the two gonad areas of the embryo and to the gonads themselves in later stages. It is well known that in many vertebrates the vascularization of the two adult gonads is not identical. As already indicated above, the analysis of this situation requires additional facts. That the nature and amount

of the blood supply to the differentiating gonad may be of importance to the type of resulting gonad is indicated by several facts, but we here cite only one. Meyns observed within testis tissue of a younger frog transplanted upon an older member of the same species that a few ova developed within the transplanted testis. The common experience with tissue transplants warrants the deduction that the testis cells which later developed into ova had meanwhile been forced to undergo a period of rigorously diminished blood supply. Such a reduction of the blood supply is a circumstance which should tend to bring about a lower metabolic rate within many of the cells of the transplant; and a lower metabolic rate is the precise condition which has earlier been found by the writer to characterize both the female organism and the female sexual cells.

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THE SCIENTIST AND AN INTERNATIONAL LANGUAGE.

By ROLAND G. KENT,

(*Read April 24, 1924*)

From the time of the philosophers Descartes and Leibnitz, men of various lands have sought to contrive a language for international or universal use; they have put forward more or less perfected sketches of such media of communication, to the number of over one hundred, but nothing that has received general acceptance. Yet the problem that they have been attacking is a very real one, which cannot be ignored by the scholar nor by the statesman or the merchant.

What I wish to say to-day, however, concerns but one aspect of this problem: namely, in how far the scientist, and I mean by this the scholar in natural or physical sciences especially, has need of an international language, what services he has a right to expect from it, what type of language is best fitted for the task, and to what extent it can satisfy his just expectations.

It is to within a comparatively recent time that the scholarly and scientific world still had an international language. There was a language used in common by many scholars of different countries and of different tongues; for example, by the Pole Copernicus (1473–1543), by the Swiss Bauhin (1560–1624), by the Italian Galileo (1564–1642), by the German Kepler (1571–1630), by the Englishman Hobbes (1588–1679), by the Frenchman Descartes (1596–1650), by the Englishman Milton (1608–1674), by the Irishman Robert Boyle (1627–1691), by the Englishmen Locke (1632–1704) and Newton (1642–1727), by the German Leibnitz (1646–1716), by the Swede Linnæus (1707–1778) by the Italian Galvani (1737–1798): if they did not write their important works in Latin, but in the local vernacular, they would immediately thereafter translate them into Latin, to make them available to the scholars of other countries. These are moreover men from eight countries, represent-

ing six different tongues, and the writings for whose sake I mention them here, are works in philosophy, politics and economics, mathematics, astronomy, botany, zoology, physiology: the great scholars and the great scientific works of nearly three centuries, coming down to within a century and a half of the present time. Men still living may have talked with men who knew Linnæus; and he wrote nearly all of his many volumes in Latin. Galvani's famous treatise on the effect of electricity on muscular activity was published in Latin, in 1791, thirteen years after the death of Linnæus.

But Latin is no longer used as the international language of the scientist. More and more it was abandoned for the vernacular of the country—French, English, Italian, German. Now as long as important scientific publications were restricted even to these four languages, with Latin as a possible fifth, the situation was not unbearable. It may be a task of some magnitude to acquire a ready reading command of these languages; it is not so onerous as to make it virtually impossible, even to the scientist whose interest is not in the forms and the manifestations of human expression, but in the content of such expression. But within the last century or less, other languages have come into use for scientific publication: Spanish, Dutch, Danish, Swedish, Modern Greek. These I mention from my own experience with the fruits of research in the linguistic field: I have no doubt that the natural scientist meets them in his technical literature. I have even known or heard of scientists who had learned, or were learning, Russian, Polish, Czechish, Roumanian, that they might make available to themselves the treatises in these languages. To-day, with the recrudescence of many minor nationalities, and the revived national feeling of some larger units, caused by the Great War, we may be facing an era in which important publications will appear in Finnish, Lithuanian, Hungarian, Serbian, Irish, Turkish, Hebrew, Arabic, Hindustani, Japanese, Chinese. And, alas, the salutary practice of giving in French or in English or in German, at the close of the treatise, a summary of the argument and of its results, has almost or quite disappeared.

The burden has become too great, even for the professional philologist and litterateur, and means must be sought to lighten it. The

first notable effort in this line, which received wide consideration, was the artificial language called Volapük by its inventor, Bishop Schleyer, which ran from 1879 to 1889. In 1889 the third international congress of Volapukists was held, at which such dissensions burst out that the movement for the use of Volapük disappeared in a few months. In 1887, the American Philosophical Society had appointed a committee to report on its utility, which had brought in a report entirely unfavorable to it; the arguments presented may be seen in the report of the committee (*Proceedings of the American Philosophical Society*, XXV., 3, 13, 312; cf. XXIV., 415, 421, 436).

Since Volapuk numerous other artificial languages have been presented to the world, in the expectation that they would supplant other means of communication between speakers of different tongues. Of all these, the most conspicuous are Esperanto, the invention of Dr. Zamenhof in 1887; Ido, an offshoot of Esperanto in 1907; and perhaps the Interlingua or Latino sine flexione of Dr. Peano. During the same time, the claims of various modern languages for such international use have been presented, notably those of English and of French, sometimes those of German, of Italian, of Spanish. There has been also a strong movement to restore Latin to its former position as the medium of international communication.

Let us postpone temporarily the question as to which of these shall be the choice of the scientific scholarly world, for its international communication, and consider what qualifications and services may fairly be exacted of an international language for scientists. Certainly, as a minimum, it should be an acceptable medium for publication of technical articles; and this involves the possession of a technical vocabulary capable of expansion to meet the new needs of scientific progress, and the possession of a vocabulary with a recognized and unquestioned standard of meaning. If other more important qualities be not sacrificed, it is desirable that the international language be easy to learn and easy to understand, and that it be brief in its forms of expression, for economy of effort on the part of the learner and economy of space in printing; but for scientific purposes it is absolutely essential that there be no ambiguity in the meaning of what is set down on the written or printed page. Further, if the

scientist wishes to use his international language for spoken as well as for written intercourse, ease and brevity gain slightly in importance; but after all, the scientist needs a medium with an absolutely certain significance, and he can express himself by word of mouth, even though with slowness and painstaking, in any language which he is able to write and to read with moderate facility.

The precision which is essential in any language intended for the recording and transmission of scientific problems and solutions, is based on two factors: the facilities for precision in the meaning of the words, and the facilities for precision in expressing the relations amongst those words. Let us consider first the matter of the meanings of the words themselves, in their relation to the threefold possibility in the selection of an international language: a modern national language, such as English, French, German; a so-called dead language, such as Latin; an artificial language, such as Esperanto. These we may take as types of their respective classes. Now the words of Latin, English, French, German have significations which are by all admitted to belong to them, and cannot wilfully be altered; but the vocabulary of any recently invented language, be it Esperanto or any other, has only a subjective meaning, conferred on it by the maker of the language or by his associates and successors. The long series of works written in Latin, English, French, German, demonstrate beyond question what the meaning of a word has been and is, and how it should be used; and the summaries of these usages constitute our dictionaries. But there is not, and cannot be, such an objective basis for the words of an artificial language; no lexical scheme set up a priori can provide for all the needs of precise and unmistakable expression. It is of course a fact that the makers of artificial languages have come more and more to use as technical words those words which are in common use in the modern languages, with only such changes as their orthography and scheme of terminations requires; the "old" Esperanto *mallumigho* has been replaced by modern Esperanto *eklipso*. This is part of a movement to an alleged ease in vocabulary; Ido and some other made languages profess to use as radical or basic words only those which are found in a number of modern languages in substantially the same form and meaning.

One system even reduces the formula to this, that any word found in Latin and English or in Latin and French is acceptable as a word in the International Language. And the significance of this is that the vocabulary of Latin must be the vocabulary of our International Language. This is increasingly clear when we note that English, French, German, and other modern languages, when they need new words in technical meanings, take them almost without exception from Latin and Greek; and any Greek word, when transliterated, may be used as Latin without more ado. Even the newest of scientific toys, the radio, uses such a Greco-Latin vocabulary; the solitary exception which I have noted is the cat's whisker, easily latinized as a *saeta felina,* or, to use the actual zoölogical term, *vibrissa.*

For the expression of the relation of the words to one another in phrases and clauses, there are three main devices: terminations, auxiliary words (chiefly prepositions and auxiliary verbs), word order. The trend of linguistic development has been, in the languages of modern Europe, to decrease the use of terminations and to increase the use of auxiliaries and the function of word order. Latin, for example, made extensive use of terminations, and a moderate use of auxiliaries, while word order was reserved to show the limits of phrases and of clauses, and to indicate emphasis. French has greatly decreased the rôle of terminations and has magnified the use of auxiliaries and the importance of word order. English has, in the main, still further dispensed with terminations. German has kept more terminations than English, and has thereby been able to reserve the word order, in a certain measure, for the conveyance of emphasis and for rhetorical effects. Esperanto has a limited number of terminations which are of very wide use, being found in virtually every word; it makes slight use of verbal auxiliaries, but a large use of prepositions; and the word order, while in the main like that of English, has a smaller fundamental value.

Were we to seek which of these three devices expresses the extreme of precision, we might be at a loss to decide. A complete system of terminations, wherein none has a twofold value, would be a perfect scheme; so would a complete system of auxiliary words, placed in a definite position with regard to the words modified. Ex-

tensive use of word-order involves an extensive use of auxiliary words; these two methods cannot therefore be employed alone. But no language employs one method only to express relations; and any system of auxiliary words, complete enough to convey all possible modifications of the thought, would be at least as cumbrous as a good system of terminations. Moreover, we may with propriety make our choice among those languages merely which are existent, and not seek for a theoretically perfect one, on which there would be no unanimity of agreement: witness the rival activities of the partisans of Esperanto, of those of Ido, of Occidental, of Latino sine flexione, of Ro, and of many others.

Now it has been urged repeatedly that the tendency of modern languages is away from the inflectional or synthetic type, such as Latin, and toward the analytic type, such as English, wherein terminations are replaced by auxiliary or relational words. This is true; but whether it represents an advance in the machinery of expression, is quite another matter. The ancient Greek, a language more highly inflected than Latin, and yet making greater use of relational words, has been recognized as one of the supreme products of the human mind, perhaps the very best medium ever in use for the precise recording and transmission of thought. Latin is another highly inflected language with a notable achievement in the same direction. French, as a written language, is still rather highly inflected; and it is a vehicle for a precise utterance whose brilliance is perhaps the product of the mentality of its users rather than of anything inherent in the form of the language. English is, in the hands of skilled writers, capable of precision, but also of many ambiguities which even the best scholars fail to note in their own composition. German of scientific content displays a definiteness of meaning which conveys the thought but does not stimulate the emotions or the imagination. Esperanto and other artificial languages have not had such careers as to establish their qualities along these lines; but they display inevitably the national idiom of the writer, be he German, French, or English, and often need retranslation into his idiom before the exact meaning can be determined.

May we not regard a reasonably full set of inflectional endings as a valuable check to misunderstanding of the meaning? An Eng-

lish-speaking person will naturally understand the implications of English word-order, which vary from those of order in French and from those of order in German, though not in the fundamentals; but there is no reason to infer that the principles common to these three languages are common to other languages of very different types. It is quite true that substitutes can be found for endings as indications of tense, voice, person, number in verbs, of case, number, gender in nouns and in adjectives; but not infrequently these devices betray their inefficiency, where endings cannot be mistaken. "I like John better than James": we need to know whether James is nominative or accusative. "He jumped into the car, covered with dust": an ending on the participle would show whether the man or the car was dust-covered. "Seated by my window, a parade went by": the employment of an inflectional ending will automatically correct the hanging participle. Many other similar instances might be quoted, to show the greater efficiency of an inflecting language over a language with an ultimate minimum of significant terminations. It may be granted that there is a duplication of the signs of interrelation of the words, and that in any language there are sure to be terminations which are identical with each other and therefore not thoroughly perspicuous in meaning; but with all that, for purposes of absolute precision, I do not hesitate to declare for an inflected language as the best means for recording the results of scientific advancement and of communicating them to a world in which unhappily there is a wide diversity of languages.

This line of thought, which might be amplified to much greater length by fuller argumentation and presentation of evidence, obviously leads to Latin as the proper medium for the scientist who wishes to address himself to an international public. We have already seen that until less than two centuries ago Latin was the recognized medium for such use; in some fields, it still remains so. In zoology, the only recognized scientific name of an animal is a Latin one, consisting of two parts, a generic and a specific one; all names of larger groups, such as families, orders, and so on, are in Latin. The botanist similarly names his plants in Latin, species, genus, family, everything; he has even, for his own benefit and protection,

ruled that the first description of a newly discovered plant, to be considered valid, must be couched in Latin. The anatomists, after wandering into a bewildering jungle of technical terminology, simplified it some thirty years ago by turning every designation into Latin, to be used in all lands, whatever the language. The physician writes his prescriptions for the pharmacist in Latin, to guard against errors in the less objectively expressed national tongue. The chemist expresses his chemical reactions in algebraic formulas which draw their significant letters from the Latin names of the elements. Even where the use of Latin is less apparent, it is none the less real; for every branch of learning makes its new technical terms from the stock of roots and words preserved to us in Latin and in Greek—and every Greek word, as we have said, is usable in Latin by a simple transliteration. In view of this, it is a quite untenable stand that some have taken, that Latin is not capable of expressing the needs and the ideas of a modern civilization, altering and progressing almost day by day.

Latin has moreover the advantage of a reasonably phonetic system of writing, which it shares on approximately even terms with German and with Esperanto and other artificial languages, while English and French show very imperfect correspondence between the written and the spoken forms of the language.[1]

Thus Latin has, as the International Language for the Scientist, manifold qualifications: a vocabulary which is essentially international; a technical terminology which is already extremely wide and is capable of easy and indefinite extension; an objective standard of word-meaning; complete precision in indicating the relations of the words to one another; a phonetic alphabet; a career of international use which has even to-day not come to an end. Besides this, Latin is, of all non-native languages, that language which is most studied in the schools of Europe and of America; and its use would not inflame international jealousies.

For scientific use, as for all general international use, some limitations might be laid. It is not to be expected nor to be desired that

[1] On this point, and other features of the problem, I have expressed my views more fully than would be relevant in the present paper, in my pamphlet "Latin as the International Auxiliary Language," pp. 31, published in 1923 by the American Classical League, Princeton, N. J.

an international language should be couched in complicated sentences, imitating for example the periodic structure of Cicero's writings; but there is no inherent reason why Latin has to be written in this fashion. The writings of scholars in the Middle Ages often show a simpler form, with shorter sentences and fewer clauses; simpler Latin, but not for that reason other than good Latin. A slight increase in the use of the prepositions, where usage is more or less optional, might be urged also. In this fashion Latin stands supreme as a means of conveying scientific thought to an international public.

To what extent, finally, are we to expect the use of an international medium for scientific publication? After all, it is unthinkable that all scientific writing should be couched in the language chosen for this purpose, be it Latin or English or Esperanto; it would not be worth while. Comparatively little technical writing in any field, whether natural scientific, or historical, or linguistic, has a permanent value or changes the course of human thought. Most scholars, in any case, would prefer to assure themselves of an immediate audience among their own compatriots, rather than risk losing that audience in part for the doubtful favor of a hearing in other lands. And so I am not those who would catch a vision of an International Language which is designed as an Auxiliary to the native tongue, but will in time spread so widely that fond parents will speak it to their infants in the cradle rather than use the déclassé vernacular. No; the International Language will not reach this point. But, for the Scientist, it may well and advantageously serve as the language of international journals, presenting in abstract the results of studies and investigations in all lands, with occasional longer articles of truly lasting worth; it may serve as the language of those epoch-making books which from time to time appear and change the aspect of the field which they treat. Such use would, I am sure, be a boon to all scholars who are now victims of a modern Babel.

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SOME PROPERTIES OF SIMPLE ELECTRIC CONDUCTING NETWORKS.

By A. E. KENNELLY.

(Read April 26, 1924.)

It is known that if in any alternating-current network of electric conductors, such as that shown in Fig. 1 (with or without transformers in the meshes), two pairs of terminals are selected,

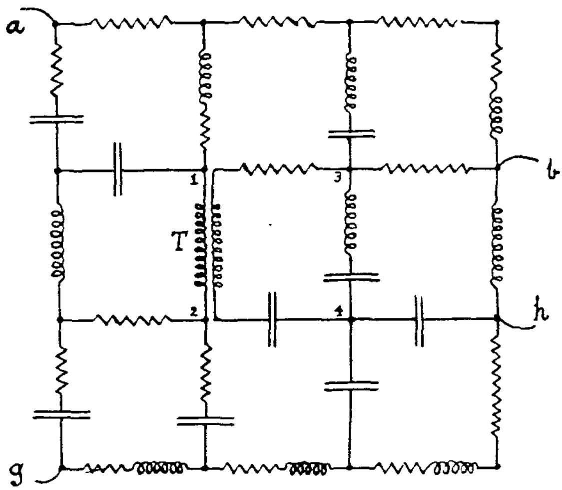


FIG. 1. Diagram representing a network of alternating-current elements.

such as a, g and b, h; then, at any given frequency, the system may be regarded as possessing, and as being defined by:

(1) a hyperbolic angle θ, in general complex (hyperbolic radians or hyps. $\angle$);
(2) a "geomean" surge impedance z_{ab} (ohms $\angle$);
(3) an "inequality ratio" q (numeric $\angle$).

Moreover, the system may be regarded as being reducible, either to the dissymmetrical T of Fig. 2, or to the equivalent dissymmetrical Π of Fig. 3, with respect to the two pairs of selected terminals, and the selected impressed frequency. The numerical values of θ, z_{ab}, and q are obtainable theoretically from three

measurements of the impedance of the network. In practice, however, it is preferable to make four impedance measurements; viz., two (R_{af} and R_{ag}) at the a, g terminals, with the pair b, h opened (freed), and shorted (grounded) respectively, followed by a

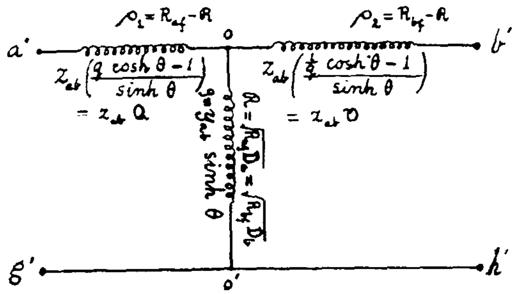


FIG. 2. Dissymmetrical T replacing a network with respect to two pairs of terminals.

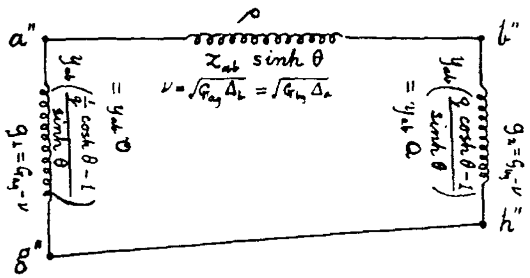


FIG. 3. Dissymmetrical Π replacing a network with respect to two pairs of terminals.

similar pair (R_{bf} and R_{bg}) at the b, h terminals, with a and g freed and shorted respectively. The angle θ is then defined by the relation

$$\tanh \theta = \frac{R_{ag}}{R_{af}} = \frac{R_{bg}}{R_{bf}} \qquad \text{numeric } \angle. \quad (1)$$

The apparent surge impedance on the a side becomes $z_{oa} = \sqrt{R_{af} \cdot R_{ag}}$ ohms $\angle$ and that on the b side $z_{ob} = \sqrt{R_{bf} \cdot R_{bg}}$ ohms $\angle$. The geometrical mean of these two gives $z_{ab} = \sqrt{z_{oa} \cdot z_{ob}}$ ohms $\angle$; while their root ratio gives the inequality ratio $q = \sqrt{z_{oa}/z_{ob}}$. From θ, z_{ab} and q, the equivalent T and Π of the network with respect to these particular two pairs of terminals can be found, as in Fig. 3.

It is proposed in this paper to discuss a few elementary cases of networks, from the point of view above outlined.

CASE I. *The terminals b and h are brought indefinitely close to the terminals a and g.* ($\theta = 0$.)

This case is represented in Fig. 4, where the impedances of the network are conventionally shown by simple straight lines. The

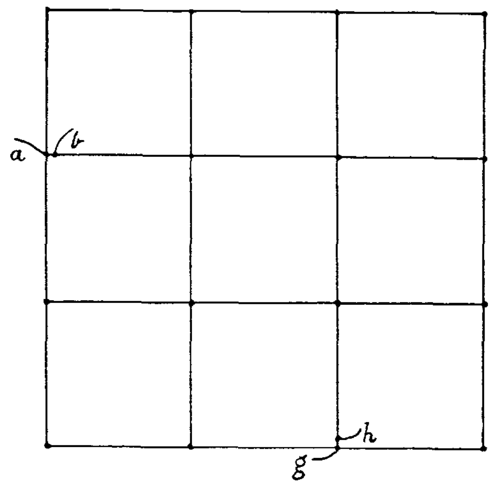


FIG. 4. Case of a network having the two pairs of terminals a,g and b,h brought infinitely close together ($\theta = 0$).

first pair of terminals, a and g, having been selected, the second pair, b and h, are placed almost in contact with the same. Then the impedances R_{af} and R_{bf} are equal, and may be represented by $\mathcal{R}$

$R_{af} = R_{bf} = \mathcal{R}$ $R_{ag} = R_{bg} = 0$ $\theta = 0$ $z_{0a} = z_{0b} = z_{ab}$

$y_{0a} = y_{0b} = y_{ab}$ $q = 1$

FIGS. 5 and 6. Corresponding equivalent T and Π for Case of Fig. 4.

ohms $\angle$, Fig. 5, the vector reciprocal of which is the admittance g mhos $\angle$. Shorting at either pair of terminals reduces the impedance at the other pair to zero. That is, $R_{ag} = R_{bg} = 0$. Hence $\theta = 0$, or the angle of the network becomes vanishingly small, and $z_{ab} = 0$, or $y_{ab} = \infty$. Also $q = 1$; and there ceases to be inequality. The corresponding equivalents T and Π of the system are shown in Figs. 5 and 6, where the series elements are indefinitely small. The impedance $\mathfrak{R}$ is the impedance offered by the network, at the selected frequency, as measured from either pair of terminals.

CASE II. *The two pairs of terminals are in "conjugate" relation to each other, or are electrically indefinitely remote.* ($\theta = \infty$.)

Fig. 7 represents a particular case of a balanced Wheatstone bridge or quadrilateral. Here the network is reduced to four elements *ab* and *ah*, each of

$$1000 + j2000 = 2236 \angle 63° \, 26' \, 6'' \text{ ohms};$$

also *bg* and *hg*, each of

$$500 + j1000 = 1118 \angle 63° \, 26' \, 6'' \text{ ohms}.$$

Under such conditions, it is evident that the application of e.m.f. to the terminals *ag* will produce no potential difference between the terminals *bh*, and reciprocally. If we measure impedances between terminals *ag*, we obtain:

$$R_{af} = R_{ag} = 750 + j1500 = 1677.05 \angle 63° \, 26' \, 6'' \text{ ohms} \angle$$

and

$$\sqrt{\frac{R_{ag}}{R_{af}}} = \tanh \theta = 1.$$

$\theta = \infty$, or the angle of the network becomes indefinitely large; while $z_{oa} = 750 + j1500$ ohms. Similarly, measuring impedances between terminals *bh*, we have

$$R_{bf} = R_{bg} = 666.\dot{6} + j1333.\dot{3} = 1490.71 \angle 63° \, 26' \, 6'' \text{ ohms}.$$

Again

$$\sqrt{R_{bg}/R_{bf}} = \tanh \theta = 1.$$

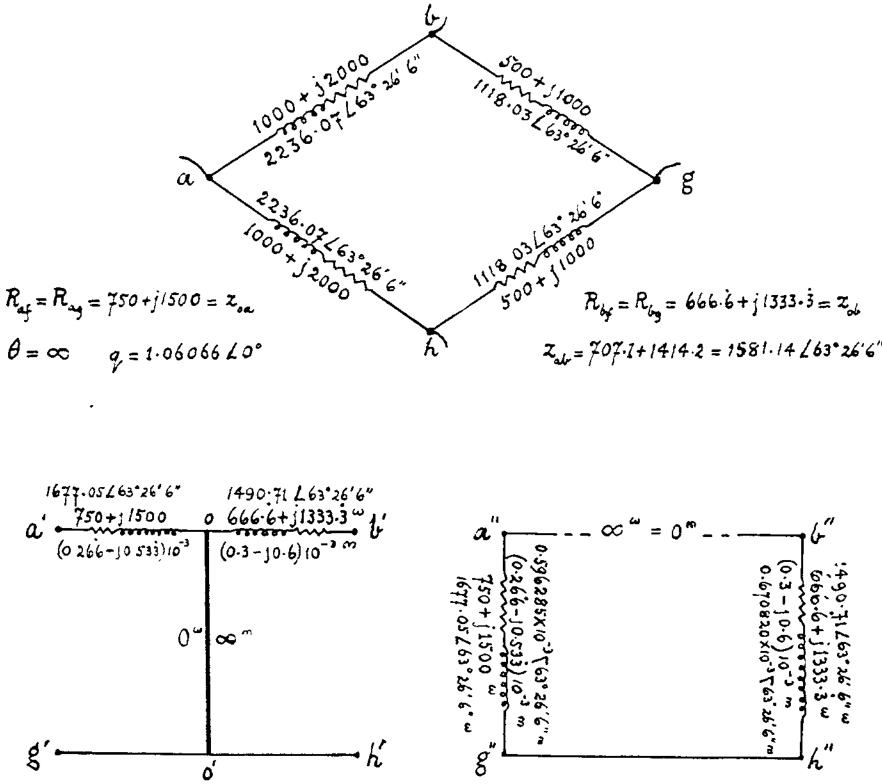
Hence

$$z_{ab} = \sqrt{z_{oa} \cdot z_{ob}} = 707.1 + j1414.2 = 1581.14 \angle 63° \, 26' \, 6'' \text{ ohms}$$

and

$$q = \sqrt{z_{oa}/z_{ob}} = 1.06066 \angle 0°.$$

Fig. 8 shows the equivalent dissymmetrical T of the Wheatstone balanced bridge, with respect to the two pairs of terminals. The

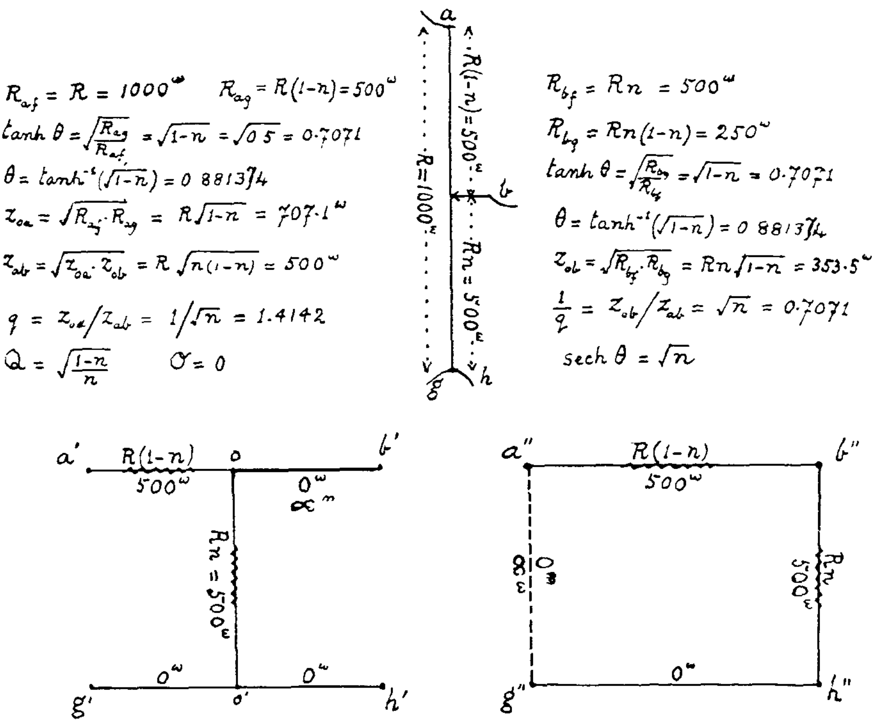


FIGS. 7, 8 and 9. A Wheatstone Balance, with its equivalent dissymmetrical T and Π. Conjugate Conductor Case ($\theta = \infty$).

staff impedance oo' is zero, or the staff admittance is infinity. This infinite leak at oo' prevents any current supplied from one side from affecting conditions at the other. Fig. 9 shows the corresponding dissymmetrical Π. In this case, the architrave impedance $a''b''$ is infinite, or its admittance is zero, which introduces a complete electrical barrier between the two sides of the system.

CASE III. *The two pairs of terminals are in simple "drop-wire" relation, the position of the contact point being variable at will.* ($\theta = 0$ *to* $\theta = \infty$.)

Fig. 10 presents the simple continuous-current case of a straight uniform wire ag, having a total resistance R, which is here assumed as 1000 ohms. The terminal h is made coincident with g; while



FIGS. 10, 11 and 12. Simple Three-Terminal Drop-wire System, with its equivalent dissymmetrical T and Π. θ varies from 0 to ∞, as n varies from 1 to 0.

terminal b is a contact, movable along the wire, so as to include Rn ohms between b and h. As shown, $n = 0.5$, or bh includes 500 ohms. Then $\tanh \theta = \sqrt{1 - n}$. When $n = 1$, or b coincides with a, $\tanh \theta = 0$ and $\theta = 0$. Again if $n = 0$, or b coincides with h, $\tanh \theta = 1$ and $\theta = \infty$. In the case indicated $\theta = 0.88137$ hyp. The geomean surge impedance $z_{ab} = R\sqrt{n(1 - n)}$, in this case 500 ohms. The inequality ratio $q = 1/\sqrt{n} = 1.4142$.

For any such drop-wire system, $\theta = 1$ when $n = 0.42$ very nearly; *i.e.*, when the impedance of bh is very nearly 42 per cent. of the total impedance ah.

SIGNIFICANCE OF THE HYPERBOLIC ANGLE θ OF A NETWORK.

As has already been pointed out, any and every network of electric conductors operated at a single frequency in the steady state, between a pair of driving or sending-end terminals a, g and a pair of driven or receiving-end terminals b, h, possesses or subtends a certain complex hyperbolic angle θ. The principal significance of this angle relates to the sending-end and receiving-end impedances of the system, with respect to the two pairs of terminals. Thus the driving-point impedance or sending-end impedance is: with the receiving-end terminals bh open,

$$R_{af} = z_{oa} \coth \theta = qz_{ab} \coth \theta, \qquad \text{ohms } \angle, \quad (2)$$

with the same terminals b, h joined or shorted,

$$R_{ag} = z_{oa} \tanh \theta = qz_{ab} \tanh \theta, \qquad \text{ohms } \angle, \quad (3)$$

and with the same terminals b, h connected through an impedance σ ohms $\angle$,

$$R_{a\sigma} = z_{oa} \tanh (\theta + \theta') = qz_{ab} \tanh (\theta + \theta'), \qquad \text{ohms } \angle, \quad (4)$$

where

$$\theta' = \tanh^{-1} (\sigma / z_{ob}), \qquad \text{hyps } \angle. \quad (5)$$

Thus, in the simple drop-wire case of Fig. 10, if a load of $\sigma = 200$ ohms is applied across the receiving-end terminals b, h, the position angle θ' becomes $\tanh^{-1} (200/353.55) = 0.64116$ hyp., and that of the sending-end terminals $(\theta + \theta')$ becomes 1.52253 hyps. The sending-end impedance $R_{a\sigma}$ is then, by (3), $707.1 \times \tanh 1.52253 = 642.86$ ohms. This result can be obtained more readily, without reference to θ, by applying a terminal load of $\sigma = 200$ ohms to the terminal b' of the equivalent T in Fig. 11, or of the equivalent Π in Fig. 12, owing to the fundamental simplicity of the system in Fig. 10; but in the general case of a complicated network, the hyperbolic angle θ has considerable advantage in computation.

Again, the receiving-end impedance of a network loaded with σ ohms $\angle$ at the terminals b, h is:

$$\begin{aligned} Z_{lb} &= z_{ab} \sinh \theta + q\sigma \cosh \theta, && \text{ohms } \angle, \\ &= q(z_{ob} \sinh \theta + \sigma \cosh \theta), && \text{ohms } \angle. \quad (5a) \end{aligned}$$

If $\sigma = 0$, or the terminals b, h are shorted,

$$Z_{lb} = z_{ab} \sinh \theta, \qquad \text{ohms } \angle. \quad (6)$$

Thus, with a load of $\sigma = 200$ ohms applied at the receiving-end terminals of Fig. 10, as before, $Z_{lb} = 500 \sinh 0.88137 + 1.4142 \times 200 \cosh 0.88137 = 500 + 400 = 900$ ohms; so that an e.m.f. of 1 volt applied at the terminals a, g would deliver a current of 1/900 ampere through the load σ, across terminals b, h. This is readily checked by inspection of Figs. 11 and 12.

CASE IV. *A transformer is included in the network.*

When a transformer is included in the network, as at T in Fig. 1, it has long been known [1] that it might be replaced by a certain equivalent conductive relation. So long as the secondary-circuit system is separated from the primary system outside of the transformer, the conversion from inductive to conductive connection is ordinarily easy. When, however, as in Fig. 1, the secondary system is joined to the primary system, or is electrically tested for phase relations in reference to the primary system, some discrepancies present themselves that need to be cleared up, in order to reach a definite and unambiguous result. Some ambiguity is to be found in recent literature on the subject. It seems that only very recently[2] the effects of reversing the connections between primary and secondary windings, on the equivalent T, have been pointed out. It is hoped that the following discussion may aid in enabling general agreement to be reached.

An "ideal" two-winding transformer may be defined, for present purposes, as a transformer devoid of power losses and of magnetic leakage. It will, therefore, have vanishingly small resistance in either winding, and no hysteresis in the magnetic circuit. All the magnetic flux links with all the turns. The inductances of the windings being $\boldsymbol{L}_1$ and $\boldsymbol{L}_2$ henries, respectively, their mutual inductance will be $\mu = \pm k\sqrt{\boldsymbol{L}_1\boldsymbol{L}_2}$ henries, where k, the coupling coefficient, is unity. The algebraic signs of $\boldsymbol{L}_1$ and $\boldsymbol{L}_2$ are essentially positive; but that of μ is open to either positive or negative interpretation. The impedance of the windings at an impressed angular velocity ω radians per second, where $\omega = 2\pi f$, and f is the frequency in cycles per second, will be $z_1 = j\boldsymbol{L}_1\omega$, and $z_2 = j\boldsymbol{L}_2\omega$ ohms, respectively; while the mutual impedance will be $M = \pm j\mu\omega$ ohms, either sign being available. We also assume that, in an ideal

[1] Bibliography 2.

[2] Bibliography 18.

transformer, the capacitance, either between the two windings, or between turns in one and the same winding, is negligible in its effects; so that when one winding is supplied with a sinusoidal current of I rms. amperes, the e.m.f. thereby induced in the other winding is $MI = \pm j\mu\omega I$ rms. volts, in quadrature with the current, the sign being taken as either positive or negative, according to the direction of reference adopted.

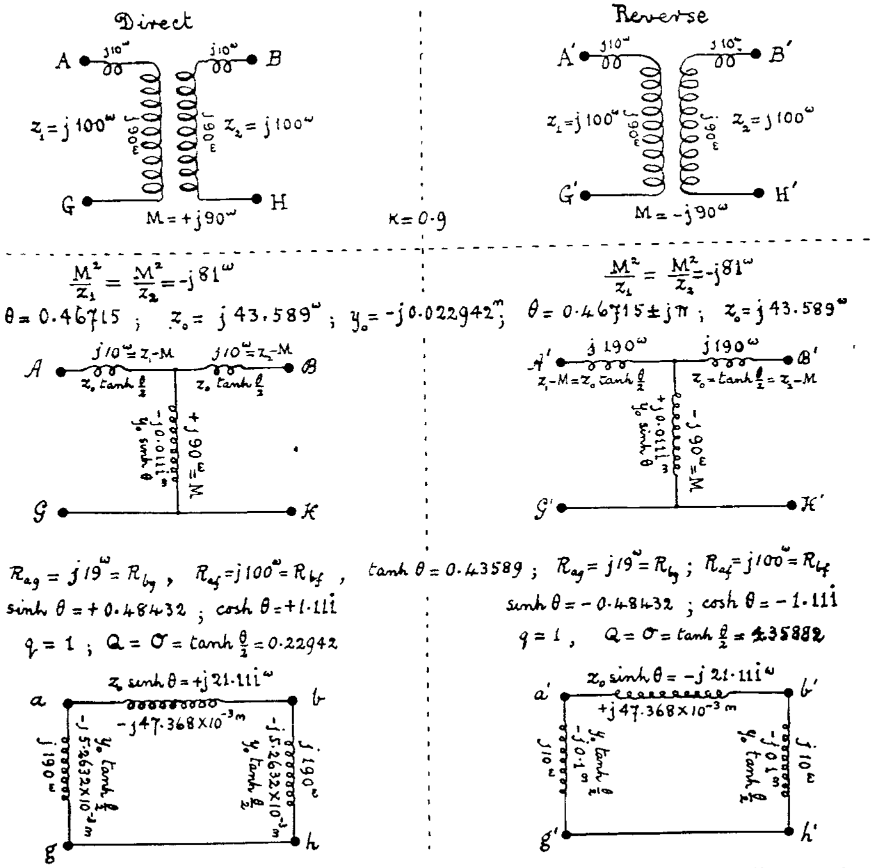


FIG. 13. Pure Level Transformer with 10 per cent. magnetic leakage, direct and reverse connections.

An actual transformer, with its two low-resistance windings suitably laid on together over a wooden toroidal core, may furnish, for many purposes, a satisfactory approximation to an ideal trans-

former, when operated at a low frequency. When, however, the coupling coefficient k falls materially short of unity, the transformer, otherwise regarded as ideal, may be called a "pure" transformer; *i.e.*, as free from power losses, or capacitance disturbances.

A transformer with equal numbers of primary and secondary turns, or unity ratio of transformation, is sometimes described as a "level transformer." Consequently, a level pure transformer has no power losses but has magnetic leakage. Consequently, $z_1 = j\boldsymbol{L}_1\omega = z_2 = j\boldsymbol{L}_2\omega = j\boldsymbol{L}\omega$ ohms, and $M = \pm j\mu\omega = \pm jk\boldsymbol{L}\omega$.

Fig. 13 indicates the connections of a level pure transformer, with 10 per cent. of magnetic leakage, or a coupling coefficient $k = 0.9$. Each winding has zero resistance, and $j100$ ohms reactance, of which $j90$ ohms is in that part of the winding completely interlinked with the other, with $j10$ ohms in the remaining part which is magnetically dissociated. On the left-hand side marked "direct" the mutual impedance is taken as $M = +j90$ ohms, and on the right-hand side, marked "reverse," as $-j90$ ohms, the second winding being reversed in the two cases with respect to the two pairs of terminals AG and BH.

With one pair of these terminals open, say BH, the impedance offered by the winding connected to the other pair AG will be $R_{af} = z_1 = j100$ ohms. Closing the second circuit by shorting terminals BH will increase[3] the impedance, as measured at AG, by an amount equal to $M^2/z_2 = -j81$ ohms. There is no ambiguity in the sign of this increment, since it depends on M^2. It is the same in either the direct or reverse connection, and in either winding of each. Consequently, in all four cases, $R_{ag} = j100 - j81 = j19$ ohms. Moreover, by symmetry, $q = 1$ for a level transformer, and the geomean surge impedance $\sqrt{R_{af} \cdot R_{ag}}$ becomes $z_o = j43.589$ ohms.

Taking the ratio R_{ag}/R_{af}, or 0.19, as $\tanh^2 \theta$, where θ is the angle of the transformer, we obtain $\tanh \theta = 0.43589$.

Owing to the double sign of M, we find that there are two values of the angle θ, corresponding to the relation $\tanh \theta = 0.43589$. One is $\theta = 0.46715$ hyp., a "real" angle corresponding to the $+$ sign of M, and the other is $\theta = 0.46715 \pm j\pi$ hyp., a complex angle, with a circular component of π radians, or 2 quadrants.

[3] Bibliography 1.

This complex θ corresponds to the $-$ sign of M. To these two different angles correspond the two different equivalent T's, and the two different equivalent Π's, shown in Fig. 13. In the T's, the staff or shunt impedance is M, with its appropriate sign: while in each T branch is the impedance $z_1 - M$, or $z_2 - M$, to correspond. In the direct-connection T, the branch impedances are $j10$ ohms each. In the reverse-connection T, they are $j190$ ohms each. Moreover, if there were no magnetic leakage ($k = 1$), the direct T branches would have zero impedance, and the reverse T branches would be $2M$, or $j200$ ohms each.

It does not seem possible to discriminate between the two T's or the two Π's, by any regular tests that can be applied to this transformer, in which the secondary circuit is kept distinct from, and independent of, the primary. That is, the sign of M remains ambiguous, so long as the primary and secondary systems are independent. Either the direct or the reverse T may be used, with any primary source on one side, and any impedance applied as a secondary load to the other. In other words, one cannot tell whether the connection is direct or reverse, and the terminals may be reversed, on the primary or on the secondary side, without affecting the impedances, voltages, currents or powers on either side.

If, however, we interconnect the primary and secondary sides of the transformer, either conductively, or through another transformer, the resulting physical conditions remove the ambiguity, and determine the sign of M. Either the direct connection, or the reverse connection, is implied, and one only of the two T's, or of the two Π's, must be used, to represent the behavior of the transformer. To take a simple case, we may connect terminal A to terminal B, and likewise terminal G to terminal H. This will place the two windings in parallel connection. If the connection is direct, an examination of the equivalent T, $\mathcal{ABGH}$, shows that the joint impedance of the two windings will be $\frac{j10}{2} + j90 = j95$ ohms; whereas, if the connection is reverse, the other T, $\mathcal{A'B'G'H'}$, shows the joint impedance to be $\frac{j190}{2} - j90 = j5$ ohms, or 19 times less. In the direct case, the current received by the parallel-

connected transformer, from 100-volt a.-c. mains, would be $-j1.053$ amperes; a simple excitation current; while in the reverse case, it would be $-j20$ amperes, which would be a short-circuit current. Moreover, if there were no magnetic leakage ($k = 1$), the parallel-connected level ideal transformer would take 1 ampere in the direct-connection case, and an infinitely great current in the reverse-connection case.

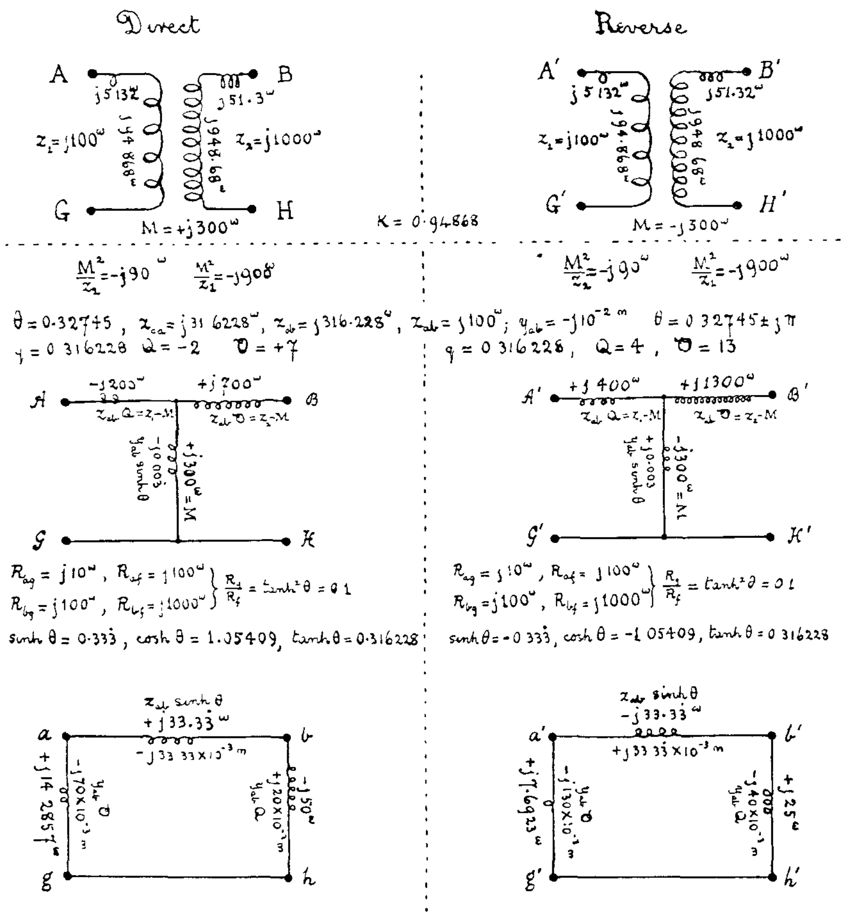


FIG. 14. Pure Unlevel Transformer, direct and reverse connections.

Again, if in Fig. 13 we connect together terminals G and H, so as to put the two windings in series, the impedance measured

between the end terminals A and B will be found, from an examination of the equivalent T's, to be $j20$ ohms with direct connection, and $j380$ ohms with reverse connection. The difference between these two measurements ($j360$), as is well known, is four times [4] the mutual reactance M. Similar remarks apply to the cases indicated in Figs. 14 and 15.

Referring to the network of Fig. 1, with the transformer T embedded in it, the interconnection between its primary and secondary systems dispels the ambiguity as to the sign of the mutual inductance μ and of the mutual impedance M. One or other of the corresponding equivalent T's will be rendered necessary, according as the connection of the transformer is direct or reverse. It might not be necessary to ascertain which connection was actually used. For two given pairs of terminals, such as a, g and b, h, Fig. 1, the values of R_{ag} and R_{af}, R_{bg} and R_{bf} should give the correct final equivalents T and Π of the entire system, transformer included; but in analyzing the composition of the final T and Π, either the direct or the reverse T and Π of the transformer, considered separately, would have to be used.

Consequently, although in dealing with a complicated network, like Fig. 1, it may be neither necessary nor desirable to know which connection is used in the transformer; yet, in order to replace the transformer by an equivalent T in the network, say between terminals 1, 2, 3 and 4, it would become necessary to ascertain, say by a test of the transformer temporarily disconnected from the network, whether the direct T or the reverse T should be employed.

In Fig. 14, the pure transformer indicated is unlevel, and has a transformation ratio of 10, with a coupling coefficient $k = 0.94868$; so that $z_1 = j100$ ohms, $z_2 = j1000$ ohms, and $M = \pm j300$ ohms, with $+j300$ for direct, and $-j300$ for inverse connection. The impedance due to the secondary closed circuit in the primary is $-j90$ ohms on the z_1 side and $-j900$ ohms on the z_2 side. Here $z_{ab} = j100$ ohms, and $q = \sqrt{0.1}$. The value of $\tanh\theta$ being 0.31623, the two values of θ are 0.32745 and $0.32745 + j\pi$ hyps., respectively, according to the $+$ or $-$ sign of the mutual inductance μ.

[4] Bibliography 12 and 13.

If two transformers, of the type shown in Fig. 14, were operated as step-up transformers, in parallel for 100-volt mains, and with their secondary terminals joined, they would operate in parallel, sharing the exciting current, provided that both were direct connected, or else both reverse connected. If, however, one were direct and the other were reverse, they would act as mutual short circuits to each other. In any of these cases, the distribution of

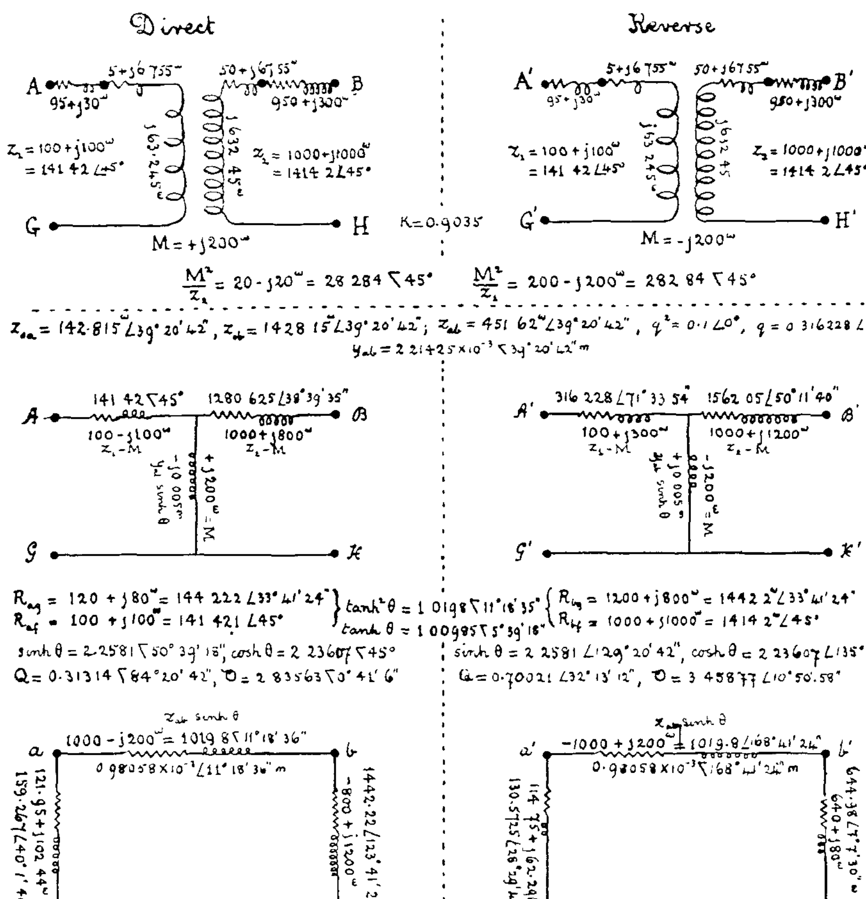


FIG. 15. General Unlevel Transformer with Air Core and External Impedances. Direct and reverse connections.

voltage and current in the system might be found by suitable connection of the T's and Π's of Fig. 14 with an appropriate source.

Fig. 15 represents the case of a general type of transformer, without core losses, but with resistance in the primary and secondary circuits, both internal and external. This case might correspond to an ordinary power transformer of 10 : 1 transformation ratio, if the shunt leak or staff of the T had resistance as well as reactance, to represent the loss of power in the magnetic circuit. The angle of the transformer is complex in this case; $\theta = 1.50156 + j2.3066$ for direct connection, and $1.50156 + j5.4482$ for reverse connection, the two differing by π radians, or 2 quadrants, in the circular component. The angle θ can ordinarily be found by charts to a fair degree of precision.[5]

TERMINOLOGY.

Question may be raised as to the applicability of the terms "direct" and "reverse," here suggested for the two relative connections of primary and secondary terminals. This is a mere matter of terminology, and the choice of these terms has no necessary implication in the arithmetical conditions under consideration. Whatever terms may be selected, they should not involve any disparagement of either mode of connection, because for some purposes it may be desirable to use the one, and for other purposes, the other.

A connection is "direct," in the sense here proposed, when, if the transformer were ideal and level, the branch impedances of the equivalent T would be zero or the terminal A would be in *direct* connection with terminal B, as against $2M$ ohms in each branch when reverse. The angle θ of the transformer would be real for direct, as against complex for reverse, connection. The staff impedance M of the equivalent T would be $+ j\mu\omega$ direct, as against $- j\mu\omega$ reverse. The two windings would be applied in the same sense and towards the same parts direct, as against relatively crossed or with reversed wires when reverse. When the primary and secondary windings are connected in series, their total impedance is greater when connected reverse than when connected direct, by the amount $4M$.

[5] Bibliography 10.

Conclusions.

1. The angle θ of a network, with respect to two pairs of terminals, may vary from 0, when the terminals are close together (a near b and g near h), to infinity, when they are very remote, or stand in conjugate relation. In a simple drop-wire circuit, the full-range movement of the contact point will cause θ to vary from 0 to ∞.

2. Any transformer may be regarded as possessing an angle θ, with respect to its primary and secondary terminals. This angle differs between the direct and reverse connection by $j\pi$ radians or $j2$ quadrants.

3. The algebraic sign of μ, the mutual inductance of any induction coil or transformer, is ambiguous. The ambiguity cannot ordinarily be cleared up, and ordinarily does not need to be, so long as the primary and secondary circuits are separated, either sign being admissible. When, however, the primary and secondary circuits are connected either conductively, or inductively through a second transformer, the ambiguity disappears, and the sign is either + or − according as the transformer connection is direct or reverse. In an ideal transformer, the + sign involves a value of $M = +j\mu\omega$ for the staff impedance of the equivalent T in direct connection and $M = -j\mu\omega$ in reverse connection.

4. Reversing either the primary or the secondary terminals of a transformer, with respect to the corresponding winding, will change the connection from direct to reverse, or oppositely.

5. Either the direct or inverse equivalent T will represent the electrical behavior of a transformer with separated primary and secondary circuits. Only one of them, corresponding to the actual internal connections, can, however, properly apply, when the primary and secondary circuits are brought into connection.

6. The direct or reverse connection of a transformer or transformers, embedded in a conducting network, may not be made evident by tests at two pairs of terminals on the network, but in order to replace a transformer in the network by the proper equivalent T, its connections must be ascertained, as to whether they are direct or reverse.

7. As a matter of terminology, the validity of the terms "direct"

and "reverse," as above suggested and defined, is debatable; but some such distinguishing terms would be useful.

List of Symbols Employed.

$D = R_f - R_s$, difference in impedances due to shorting at opposite ends (ohms $\angle$).

$\Delta = G_s - G_f$, difference in admittances due to shorting at opposite ends (mhos $\angle$).

f, frequency of sinusoidal current in a circuit (cycles per second).

G_{af}, G_{as}, admittance of a network at a terminals when freed and shorted at b respectively (mhos $\angle$).

g, admittance of the staff leak in an equivalent T (mhos $\angle$).

g_1, g_2, admittances of the pillar leaks in an equivalent Π (mhos $\angle$).

θ, angle of a transformer or of a network with respect to two pairs of terminals (hyperbolic radians or hyps. $\angle$).

θ', angle of a load at the receiving end of a network (hyps. $\angle$).

I, value of a sinusoidal alternating current (amperes).

$j = \sqrt{-1}$.

$k = \dfrac{\mu}{\sqrt{L_1 L_2}}$, coupling coefficient of a transformer (numeric).

L_1, L_2, inductances of the two windings of a transformer (henries).

$M = \pm j\mu\omega$, mutual impedance of a transformer (ohms $\angle$).

$\mu = \pm k\sqrt{L_1 L_2}$, mutual inductance of two transformer windings (henries).

n, the ratio of secondary to primary impedance in a simple drop-wire system (numeric $\angle$).

ν, architrave admittance of a Π (mhos $\angle$).

Π, type of equivalent circuit having one impedance in series and two terminal leaks.

$\pi = 3.14159\ldots$

$Q = \dfrac{q \cosh \theta - 1}{\sinh \theta}$, a factor in assigning the elements of an equivalent T or Π (numeric $\angle$).

$\mathcal{Q} = \dfrac{\frac{1}{q} \cosh \theta - 1}{\sinh \theta}$, a factor in assigning the elements of an equivalent T or Π (numeric $\angle$).

$q = \sqrt{z_{oa}\, z_{ob}}$, inequality factor of a network or of its equivalent T or Π (numeric $\angle$).

$\mathfrak{R}$, impedance of the staff leak in an equivalent T (ohms $\angle$).

R_{af}, impedance of a network measured from the a, g terminals with the b, h terminals free (ohms $\angle$).

R_{ag}, impedance of a network measured from the a, g terminals with the b, h terminals shorted (ohms $\angle$).

$R_{a\sigma}$, impedance of a network measured from the a, g terminals with the b, h terminals connected through an impedance of σ ohms.

ρ, architrave impedance of an equivalent Π (ohms $\angle$).

ρ_1, ρ_2, impedances of the arms or line branches of an equivalent T (ohms $\angle$).

σ, a terminal load impedance at the receiving-end terminals (ohms $\angle$).

$y_{ab} = 1/z_{ab}$, geomean surge admittance of a network or of its equivalent T or Π (mhos $\angle$).

Z_{lb}, receiving-end impedance of a network at b terminals (ohms $\angle$).

z_1, z_2, impedances of the two windings of a transformer (ohms $\angle$).

z_o, surge impedance of a symmetrical network (ohms $\angle$).

$z_{oa} = \sqrt{R_{ag} \cdot R_{af}}$, surge impedance of a network from the a, g terminals (ohms $\angle$).

$z_{ob} = \sqrt{R_{bg} \cdot R_{bf}}$, surge impedance of a network from the b, h terminals (ohms $\angle$).

$z_{ab} = \sqrt{z_{oa} \cdot z_{ob}}$, geomean surge impedance of a network or of its equivalent T or Π (ohms $\angle$).

$\omega = 2\pi f$, angular velocity impressed on a network or transformer (radians/second).

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Without pretensions as to completeness.

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THE DANISH DIALECT OF BORNHOLM.

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Read by title April 24, 1924.

INTRODUCTION.

The Island of Bornholm lies in the Baltic Sea, 115 miles east of Copenhagen, about 22 miles south of the Swedish coast, and 50 miles north of the German island of Rügen. The total area of the island, which also includes the little group of isles known as Christiansö, is 225 square miles, with a population of over 43,000. Bornholm (ON.[1] *Borgundarholmur*) was the habitat in prehistoric times of an early Nordic race and there are still a number of stone circles, many so-called *bautarsteinar*[2] and other primitive remains on the island, especially in the neighborhood of Almindingen and Gudhjem. The population developed later into predatory Vikings, who were long notorious for their marauding proclivities. In the twelfth century, Bornholm became a fief of the Archbishop of Lund, under which sway the island remained until 1510, when it was seized by the Hanseatic League, but was captured by the Danes in 1522, and returned by them to the City of Lubeck in 1526. The Swedes took the island in 1625, and their domination was assented to by Denmark in 1658 by the Peace of Roskilde. The Bornholmers, however, desired to be Danish and themselves expelled the Swedish garrison in 1660, since which date Bornholm has been an integral part of Denmark. The curious round churches built in tower-shape and completely circular, with walls often six feet thick, form one of the most interesting features of the island. These buildings, which date from the twelfth and thirteenth centuries, were undoubtedly used at an early date for purposes of defence.

The Bornholmers have always had their distinct speech, which, although popularly regarded by the other Danes as a Swedish dialect, is really the modern representative of the old East Danish

[1] Old Norse: *Borgundarhólmr*; Icel. *-holmur*, a term which in a modified form, *Borgunderholm*, was used until quite recently in Danish.

[2] *bautarsteinar* memorial stones.

linguistic group to which south Scandian, now in Sweden, also belongs. Bornholmsk shows only a superficial resemblance to true Swedish.

The distinctively Danish dialects may be classified as follows: (1) West Danish, comprising the several idioms of Jutland, whose chief peculiarity is the complete rejection of the ON. ending *-a*; (2) Island Danish, comprising the dialects of Fyen-Langeland; north and south Sjaelland, including the city idiom of Copenhagen, a refined form of which has become the standard Danish (Rigssprog), and the almost uniform speech of Lolland-Falster, all which variants substitute *-e* for the ON. *-a*; (3) East Danish: Bornholmsk and southern Scandian (*skånsk*) in Sweden, shading towards the north into real Swedish; both groups retain the ON. *-a*, in distinction from the other divisions.

Similarly to the linguistic movement on the Faeroes, there exists a less organized action on Bornholm to lay stress on the local dialect which they call a "language," because it is unintelligible to other Danes, and many purists on the island strongly resent the constantly increasing introduction of Danisms from the standard tongue. They are equally annoyed at the statement that Bornholmsk is a Swedish patois, pointing out that so different is their language from Swedish that, when Swedes come to settle on Bornholm, they never learn to speak the dialect correctly. This is perfectly true, as no real Swede can pronounce the palatalized consonants which distinguish Bornholmsk (see below) from most other Germanic idioms. The Frisian of Sylt is distinctly palatal and the ordinary soft *l* of modern Icel. is practically a palatal lingual. The B. *ń* (palatal *gn*) is, however, rare in Germanic dialects, appearing however in certain other Danish variants, as in Jutland and Fyen.

There are, so far as I know, no societies devoted to the preservation of Bornholmsk, for which reason the dialect will in the course of time be crowded out by the Standard Danish of the government schools, a process which has already begun in the towns, particularly in Rönne, the capital of the island. The most striking popular defence of the local idiom is that of P. Möller, a landscape gardener, who in his "Bornholm Language" [3] laments the

[3] *Det Bornholmske Sprog af* P. Møller, *Havearkitekt og Landskabsgartner*, Rønne, 1918.

decay of the old words and forms and inveighs against the "snobbish" desire of the younger people to assimilate their beautiful speech to the harsh and monotonous phonetics of Copenhagen. Möller's description of the dialect is, however, not scientific and, therefore, not so valuable for the purpose of record as the highly accurate phonetic treatise of Prof. Vilhelm Thomsen and that of Prof. Ludvig F. A. Wimmer on the Bornholmsk grammar, both included in J. C. S. Espersen's "Bornholm Dictionary."[4] It should be added that Bornholmsk has had two poets of considerable merit; viz., Espersen himself and J. P. Möller (d. 1891) who, although only a baker in Allinge, was a highly gifted and many-sided genius. Some of the poems of both these writers are given in the edition of the dictionary in question.[5] There is also a very good collection of Bornholm melodies assembled by H. Johansen,[6] which gives a large number of characteristic native folksongs, with words and music.

The material for the present sketch, which, so far as known, is the first description in English of Bornholmsk, was obtained during a visit to the Island from a number of personal sources, especially from natives resident at some distance from the towns. The best speaker of the dialect whom I heard was a Mrs. Jespersen, the custodian of one of the most ancient of the round churches, who was able to speak both Danish and Bornholmsk without confusion. This material has been amplified and confirmed by the above mentioned works.

PHONETICS.[7]

The Bornholmsk phonetics are rather complicated and the difficulty of recording the dialect is greatly magnified by the accepted method of writing, which, in at least one important respect, is inaccurate, as the combination *-jn* is used throughout for two

[4] J. C. S. Espersen, *Bornholmsk Ordbog, med Indledning og Tillaeg, udgivet af det Kgl. Danske Videnskabernes Selskab*, Copenhagen, 1908.

[5] See *Bornholmsk Ordbog*, pp. 150–169.

[6] *Viser i Bornholmsk Mundart, samlede af* H. Johansen, Rönne, 1911.

[7] The following abbreviations have been used: B. = Bornholmsk; conj. = conjunction; D. = Danish; def. = definite; Eng. = English; Germ. = German; Icel. = modern Icelandic; indef. = indefinite; n. = note; OD. = Old Danish; ON. = Old Norse; pl. = plural; sg. = singular; Sw. = Swedish; Wimmer = article in Introduction to Espersen's *Ordbog Bornholmsk* (n. 4).

different sounds: viz., for *ng* as in *singing* and for the palatal *gn* as in French *signe* (= *ń*). The system dates from Espersen's manuscript prepared in the early half of the nineteenth century, but corrected to a certain extent, as indicated above, by Prof. Thomsen. It should be noted, however, that some of the modern writers in the dialect now distinguish between the soft *ng*-sound and the palatal *ń*, using *ng* for the former and *jn* only for the latter vocable.

The rules for the pronunciation of Bornholmsk may be tabulated as follows: *a*, flat, as in *hat*; *ā* or *â*, as *ah*; *aa* = D. long *aa*, like Eng. *o* in *lone*; *b*, as in Eng.; never soft as medial or final, as in D.; *d* = hard *d* as initial, but nearly as *dh* (= *th* in *this*) as medial or final, except that medial *dd* is always pronounced hard and with emphasis (final *d* is sometimes omitted, as in *vad* 'what,' but it is usually omitted in writing in such cases); *e* = *e* in *met*; *ē* = *ee* in Germ. *Seele*; *f* as in Eng., often omitted in pronunciation as a medial, as *aette* = *efter* = 'after'—*ōta* = *ofta* 'often,' etc.; *g*, as Eng. hard *g*; *gj* or *ǵ* is the palatalized *g*, an approximation between *dy* and soft Eng. *j*; *h*, as in Eng.; *i* = short *i* in *pin*; *ī* = *ee* in *feel*; *j* = Eng. consonantal *y*, when B. *j* is used consonantally (but see just below); *jd* = *d'* = palatalized *d* (d^y); *jl* = *l'* = palatalized *l*, as Span. *ll*; *jn* = *ń*, as in French *signe*, but in some texts = *ng* as in *singing* (see above); *k* as in Eng.; *kj* = *ć*, palatalized *ch* as Pol. *ć*; *l*, almost like the American *l* in *well* (not so hard as the Eng. pronunciation); *m* and *n* as in Eng.; *o* = obscure Sw. *o*, almost *oo* in *good*; *ō* = Eng. *oh*; *p*, as in Eng.; *r*, always trilled on the tip of the tongue (never gutturalized as in D.); *s*, as initial = *s* in *sad*—as medial or final = Eng. *s* in *rose* (= *z*) and is usually indicated by *z* in such cases; *sj* = *ś*, a palatalized *sh* like Polish *ś* and often a trifle rougher, like an obscure Eng. *sh*; *skj* and *stj* = *sć*; viz., *s* with palatal *ć*; *t*, as in Eng.; *t'* = *ć*; *u* = *oo* in *soot*; *ū* = Sw. long *u*, like the Magyar *u* (not Germ. *ü*); *v*, hard and never slurred as in D.; *w* occurs chiefly in the combination *aw* = *ou* in *house*, sometimes written *au* (or *ou*); *y* (always a vowel) = D. *y* or Magyar *ü*; *å* = Eng. *o* in *mortal* (*aa* is used for the long vowel); *åj*, diphthong of *å* and short *i*; *ae*, when short, almost = *a* in *hat*; $\overline{ae}$ = Eng. *a* in *care*; *aej* diphthong of $\overline{ae}$ and short *i*; *aew*, diphthong

of $\overline{ae}$ and short *u*; *ö* = *i* in *bird* and sometimes short Germ. *ü*, as in D. *kod* 'meat.'

Intonation and Accent.

Bornholmsk has no glottal catch, so frequent in standard Danish and Juttish, but, like the Danish dialects of Lolland-Falster and Fyen-Langeland, uses the glided vowel exclusively. Unlike Sw., B. has no distinct musical tones, although there exists in B. a recognized rather monotonous musical intonation, which causes the Copenhageners to assert that the Bornholmers "sing." This intonation is more agreeable than the "sobbing" cadence used by the vulgar Copenhageners. There is, therefore, in B. no tonal differentiation in meaning, as, for ex., in Sw. between *axel* 'axle' and *axel* 'shoulder,' pronounced respectively *áxél* (two tones) and *áxel* (one tone). The word is *āwsel* in B. for both senses, which must be determined by context. Wimmer points out (pp. 66–67) that the stress-accent in B. differs very slightly from that of the standard D., and that, when it does differ, the stress resembles Sw. or else is entirely distinct, as in nouns ending in *-i* such as *eskeri*, where the accent may rest on the first or the last syllable indifferently; thus, *feskerí* means 'fishery' in general, but *féskeri* = 'fishing.'

It is doubtful whether Bornholmers will ever learn to pronounce D. after the standard fashion, as even the young children find the glottal catch an impossibility. Danish spoken without this unpleasing peculiarity always indicates an "outsider"; viz., either a foreigner or a rural person.

It should be noted that, when consonants are doubled in B., they must be dwelt upon in the same manner as in Italian, a phenomenon unknown in Danish, but usual in Swedish.

The following extracts, the first in the accepted spelling with the literal Danish and Swedish equivalent text, the second in the accepted spelling with Wimmer's phonetic version and Danish translation, and the third in phonetic rendering as heard by me with Danish equivalent, will serve to illustrate the dialect.

I.[8]

Bårrijnhålmarna, dūer nu i många Aar hā (1) arbājad hen te å faa (2) dårra
Bornholmerne, der nu i mange Aar har arbeidet hen til at faa deres
Bornholmarna, som nu i många år ha arbetat for att få sitt

Spraag satt paa Modan, hā, forudan dī fårra (3) hūer i Baagen omtalde
Sprog sat paa Moden, har, foruden de faa her i Bogen omtalte
språk satt på modet, ha, forutom de få har i boken omtalte

Maadarna, āw tād (4) dūejn sompla Framgangsmaadan te Jaelp, lāejefram å (5)
Maaderne, ogsaa taget den simple Fremgangsmaade til Hjaelp, ligefrem at
metoder, också tagit det enkla tillvagagående till hjalp, rent av at

kassēra ēn Maejnde Ōr, som ha syjnts dom å varra for bårrijnhålmska,
kassere en Maengde Ord, som syntes dem at vaere for bornholmske,
kassera en stor del ord, som syntes dem att vara for bornholmska,

aejle som, me ānra (6) Ōr, hā vād (4) dom for mājed forsjellia fraa dī danske,
eller som, med andre Ord, har vaeret dem altfor forskellige fra de danske.
eller som, med andra ord, ha varit dem altfor skilda från de danska.

Nu hār vaart Spraag ju i ajl āerene Tīd vād et rāejt (7) Spraag, så vi kojne (8)
Nu har vort Sprog jo fra Arilds Tid vaeret et rigt Sprog, saa at vi kan
Nu har vart språk ju från urăldriga tider varit ett rikt språk, så att vi kunna

sājtas (9) ojnvaera ijn Haaven Ōr å (3) livael hā nok te Hūzbehaw.
sagtens undvaera en Masse Ord og alligevel have nok til Husbrug.
verkligen undvara många Ord ock likval hava nog till husbehov.

Men ser (10) ijn lijd nåjara aette (11), blēr ijn snārt vār (12), a de(d) ijkje naer
Men ser man lidt noiagtere efter, bliver man snart opmaerksom paa, at de ikke naer
Men ser man lite nogare efter, blir man snart opmarksam på, att de icke

altīd e dī Orn som vi baest kojne ojnvaere (13), dī ha kassērt (14), men a di
altid er de Ord som vi bedst kan undvaere, de har kasseret, men at de
altid aro de ord som vi bast kunna undvara, de ha kasserat, men att de

mājed ōta (11) e Ōr som vi kojne hā Brog for å som nāwe kajn aerstattas (13)
meget ofte er Ord som vi kan have Brug for og som neppe kan erstattes
mycket ofta aro ord som vi kunna hava bruk for ock som knappt kunna ersattas

me aenkla danska Ōr. Ōta e de(dh) ed Or som daer ska flera danska Ōr te å
med enkelte danske Ord. Ofte er det et Ord som der skal flere danske Ord til at
med enkla danska ord. Ofta ar det ett ord som det behoves flera danska ord till att

forklāra å for manga å dom ska dūer ējndaa så gāled hēla Saetnijner te å
forklare og for mange af dem skal der endog findes hele Saetninger til at
forklara ock for manga av dem skola aven anvandas hela satningar till att

[8] *Det Bornholmske Sprog*, p. 71 (n. 3). The numerals refer to the commentary below.

forklāra. Detta e så forārjelıt, a ijn kajn blē hēlt hārmse (15) *paaed,*
forklare. Dette er saa forargeligt, at man kan blive helt vred paa det,
forklara. Detta ar så forargelsesvackande, att man kan bli helt vred på det,

naar ijn taenjnkjer saj, a de(d) e vaara ājena Landsmaejn som—i
naar man taenker sig, at det er vore egne Landsmaend som—i
nar man tanker sig, att det aro våra egna landsmän som—i

ren Forblijnelse—kār borred dom så rystoppena (16) *gāled* (17) *ad. Men de(d) e āw*
ren Forblindelse—har baaret dem saa ganske galt ad. Men det er ogsaa
ren forblindelse—ha burit sig så ganska galet åt. Men det ar också

forarjelit, a di hā kassērt soddena (18) *Or, for, sael om dessa kajn udtrykja*
forargeligt, at de har kasseret saadanne Ord, for, selv om disse kan udtrykke
förargelsesvackande, att de ha kasserat sådana ord, ty aven om dessa kunna uttrycka

Menijnen, så kajn di alri komma te å passa i vaart Spraag, men vil stödda
Betydningen, saa kan de aldrig komma til at passe i vort Sprog, men vil stöde
betydelsen, så kunna de aldrig komma till att vara passande i vårt språk, men skola stota

å skårra i Örn å gjårra vaart aejlans så naetta bårrijnhålmska Spraag styjgjara.
og hakke Örene og gore vort ellers saa behaglige bornholmske Sprog grimmere.
ock hacka oronen ock gora vårt annars så behagliga bornholmska språk fulare.

Literal Translation.

The Bornholmers, who now for many years have worked to get their language made fashionable, have, in addition to the few methods mentioned in this book, also adopted (taken) the simple procedure of rejecting a great many words which seemed to them to be too Bornholmsk or, in other words, which have seemed (been) to them too different from the Danish (words). Now, our language has been from time immemorial a rich language, so that we really can do without a lot of words and at the same time have enough left over for household use. But if one examines the case a little more closely (exactly), one will at once (soon) be aware that it is not by any means always those words which we can best dispense with that they have rejected, but that these are very often words which we could have use for and which scarcely can be replaced by single Danish words. Often it is a word which it takes several Danish words to explain and, for many of them, even whole sentences must be used to explain (them). This is so annoying that one can get very angry at it, when one considers that it is our own countrymen who, in sheer blindness, have behaved

(themselves) so stupidly. But it is also annoying that they have rejected such words, because (= for), even if these (*i.e.*, the new words chosen) can express the sense, they can never come to be suitable in our language, but will shock and irritate (= cut) (our) ears and make our otherwise so agreeable Bornholmsk language (much) more ugly.

II.[8]

God awtan, liden Elna Gods fred!
Good evening, little Elna, God's peace!
God awtan, min deilia rosa!
Good evening my beautiful rose!
ad gubbajn hajn vill freia, jâ vedd,
That the old man will court you I know,
Men toustuijn vastu jo toza.
But if you took him, you would be a foolish lass.
Te ófrol dedh lakkar vell snarara, Du!
To funeral ale it is drawing near (for him), do you see?
En konna—dedh bler nok for sijlla,
A wife—it is much too late (for that),
Men jâ går å stjarnar på piblana nu,
But I am going to peep at the girls now,

Forr jâ e på nå nu så vijlla.
For I am almost ready (to marry).
Hvad, liden Elna! Hvad min deilia roza!
What, little Elna! What my beautiful rose!

Go āwtan, līdhen Aelna, go(dh)s frēdh!
God Aften, lille Elna, Guds Fred!
Go āwtan, min dājlia rōza!
God Aften min deilige Rose!
A gobbań hań vell frājja, jā vēdh,
At Gubben vil vel fri, veed jeg,
Maen tāwstuiń, vāstu (19) *jū tōza.*
Men tog Du ham, var Du jo en Tosse.
Te efrol de' lakkar vael snārara, Du!
Til Gravöl lakker det nok snarere, Du!
En kånna—de' bler nåk (20) *får sil'l'a,*
En Kone—det bliver nok for silde,
Maen jā går å sćaernar (21) *på piblana nu,*
Men jeg gaar og kigger paa Pigerne nu,

Får jā e pånå nu så vil'l'a.
For nu er jeg naesten i Stand dertil.
Vā, līdhen Aelna! Vā, min dājlia rōza!
Hvad lille Elna! Hvad min deilige Rose!

III.[9]

Colloquial Phrases.

Sē hāer vā jā hār te dāj; praestiń vā haeruda å jā fićć de(d) udå (22) *hannòm.*
Se her hvad jeg har til Dig; Presten var herude og jeg fik det fra ham.
See here what I have for you; the priest was out there and I got it from him.

Maa jante (23) *gaa te ćaerća? Ja, maen hor vā jā sājjer dāj; kom snārt ijen te baellana* (24).
Maa jeg ikke gaa til Kirken? Ja, men hor hvad jeg siger Dig; kom snart tilbage til Bornene.
May I not go to church now? Yes, but listen to what I tell you; come back soon to the children.

[8] See N. 9.
[9] *Bornholmsk Ordbog*, p. 150 (n. 4).

Hon ńćć ed āńed sińá folde mēń (25) *jemm. Hań fore-na* (26) *mē sāj.*
Hun aendrede sin Mening og fulgde med ham hjem. Han forde hende med sig.
She changed her mind and went home with him. He took her with him.

Roserna tāva darra blā nū. Vińteriń kommer.
Roserne tabe deres Blade nu. Vinteren kommer.
The roses are losing their petals now. Winter is coming.

Hań hār īń lidiń horra (27) *å to pibla* (28). *Hań hār trē baella.*
Han har een lille Dreng og to Piger. Han har tre Born.
He has one little boy and two little girls. He has three children.

Horrin ńćć pryl (29) *à bońiń som hań mote paa vaeń.*
Drengen fik Prygl fra Bonden som han modte paa Vejen.
The boy got a beating (a cudgel) from the peasant whom he met on the road.

De(d) vā ićće (*inte*) *notti å gārra,*
Det var ikke nodvendigt at gore.
It was not necessary to do that.

Kom hāēr, gōa venner, å tān jorr en rūz!
Kom her, gode Venner, og drik Noget (*tag Jer en Rus = 'get drunk'*)!
Come here, good friends, and have a drink (with us)!

Hań taeńte paa å ģefta saj å ģićć ud å frājja å saa mote hań en pājja (30) *daer aelskadiń* (31) *mājed.*
Han taenkte paa at gifte sig og saa gik han ud at frie og traf en Pige der elskede ham meget.
He thought of getting married and so he went out and met a girl who loved him very much.

E daer vann (32) *udi ćitan* (33)? *Ja, maen de(d) maa dūnte* (34) *faa. De(d) skā hestana hā.*
Er der Vand i Spanden? Ja, men det maa Du ikke faa. Det skal Hestene have.
Is there water in the pail? Yes, but you can't have that. The horses must have it.

Lokk dorn op å lāń (35) *gaa ud.*
Lukk Dorren op og lad ham gaa ud.
Open the door and let him go out.

Vār e piblan nu? Hon fann horriń å trać mēń (25) *jemm.*
Hvor er Pigebarnet nu? Hun fandt Drengen og trak hjem med ham.
Where is the little girl now? She found the boy and went home with him.

Hań spōre māj skā jā ģē jorr iń bårrinhålmsk sång; maen de' kań jante ģārra fårr ja har forgatt al't.
Han spurgde mig, om jeg vil give Jer en bornholmsk Sang (36), *men det kan jeg ikke gore, for jeg har glemt det Hele.*
He asked me to give you a Bornholm song, but I cannot, for I have forgotten everything.

COMMENTARY ON THE TEXTS.

(1) *arbājad* 'worked'; also written *arbēad* and *arbīad*.

(2) *dårra* 'their'; used in B., as in D. as the 3 p. possessive reflexive, which in Sw. is always *sin* (m. and f.), *sitt* (n.); pl. *sina*, used for both sg. and pl.

(3) *fårra* 'few'; note *-rr-* and the redundant pl. *-a* (cf. Icel. *fáir*).

(4) *tād*, contraction for *tāged* 'taken'; cf. *vād* for *vārid* 'been' (other examples below).

(5) *å* = conj. *og* 'and,' the prep. *af* 'of' (as in *udå*, n. 23) and also the infin. particle *at*.

(6) *ānra*, pl. of *āńiń* 'other' (D. pl. *andre*). Note the elision of *d*, pronounced in both D. and Sw.

(7) The *k* is completely elided from the original *kt* = *-k*, plus neuter *-t*: *raekt* (D. *rigt*) and the subsequent palatalization of the *t* to *ć* (n. 9).

(8) *kojne* = *koñe*: pres. pl *kunne*, as Sw. *kunna*, but lost in D. = *kan* for both numbers.

(9) *sajtas* = *saćć as* = D. *sagtens* 'really'; note elision of *g* (original *gt*) and palatalization of *t* to *ć*, and total disappearance of *-n* (cf. n. 7).

(10) *ijn* = *ıń* = D. *een* 'one,' used in B. for the D. and Germ. indef. *man* (Fr. *on*). The indef. *man* is never used in true B.

(11) *aette* = D. *efter* 'after' and ON. *eptir*. Here there is no palatalization as in nn. 7 and 9. Note also B. *ōta* = D. *ofte*, with compensative long *o* for loss of *-f-*.

(12) *vār* 'aware' = D. and Norse *var* (not in use in colloquial D.) = Germ. *gewahr*.

(13) *ojnvaere* 'dispense with'; a Danism: *undvaere* in D. is a Germanism like *undvara* in Sw. = Germ *entbehren*. It should really be *undvarra* in B., as the verb 'to be,' on whose analogy the word is made, is *varra*. Note also B. *aerstattas* = D. *erstattes*, also a Germanism, from *erstatten*.

(14) *kassērt* 'rejected'; observe the absence of the vowel (*a* before the participial *t* (D. *kasseret*; Sw. *kasserat*).

(15) *hārmse* 'angry'; cf. Sw. *harmsen* 'angry' and D. *Harme* 'indignation.'

(16) *rystoppena galed* 'exceedingly stupidly'; used in B. only with *galed*; OD. *ryskgalen* 'very angry' (*rysker* means 'mad' in B.). *Toppena* = Dalicarlian *tuppande* (Espersen, 279) 'one who is excessive,' probably cognate with Germ. *toben* 'rage' (thus Espersen).

(17) *galed* 'stupid' = Sw. *galet*.

(18) *soddena* 'such,' pl. Note *o* for D. short *aa*, pron. like short Eng. *aw*; D. *saadane*, but Sw. *sådan*, with long *å*.

(19) *taustuiń* 'tookst thou him' = 'if thou didst take him' = OD. *togst Du ham*.

(20) *nåk* 'enough,' pron. almost like short *nok*, as in D.; cf. Sw. *nōg*.

(21) *sćaernar* 'stare'; cogn. with Eng. *stare* more than with *D. stirre*. The expression *pånå* = D. *naesten* 'almost' is purely B., and now almost obsolete. *Pånå* is probably a translation of Germ. *beinahe* (cf. Frisian *binai*; Sylt.).

(22) *udå hannomm*; lit. 'out of him' (see n. 5) = D. *udaf* in the same sense (cf. New York slang: *off'n him* 'from him').

(23) *jante* = *ja inte* 'I not.'

(24) *baella* 'children.' This stem is probably not *balg* 'bellows,' from the pot bellies of small children, but the same as in south Germ. *bubli*, dim. of *bube*

'boy.' It is seen also in Scotch *billy* 'boy, lad.' It is probable that the Amer. slang *Bill,* used in address for any name at all, contains the forgotten sense *boy.* The sg. in B. is *baell* with the masc. article *baelliń* 'the child.'

(25) *mēń* = *mēdiń* 'with him'; D. *med'en* = *med ham.*

(26) *fore-na* = *fore haeńe* 'he took her.'

(27) *horra* 'boy': a difficult word and purely B., thought by some to be cogn. with *herre* in Småländsk *ollherre* 'cattle-boy' (Espersen, 134), but this *herre* is probably a variant of Sw. *herde,* seen in *fårherde* shepherd; D. *Hyrde*; Germ. *Hirt* and scarcely connected with B. Note that the form *hork* 'boy' is used in North Sjaelland and also in Jutland for *boy* in an opprobrious sense. With this latter use, cf. Scotch *hawkie* 'clumsy lad.' These latter forms may be diminutives of the original stem of B. *horra.*

(28) *pibla,* pl. of *pibel* 'small girl,' undoubtedly a combination of *pi*(*ga*) 'girl' and *baell* 'child' and exactly equivalent to D. *Pigebarn* 'little girl.' A grown girl in B. is *pajja* (see n. 30).

(29) *pryl* 'cudgel, beating' = D. *Prygl.*

(30) *pajja* 'grown girl' = D. *Pige*; Sw. *piga* 'maid'; in distinction from *pibel* 'girl-child.'

(31) *aelskadiń* 'loved him'; D. colloquial: *elskede-en* (*ham*).

(32) *vann* 'water' = D. *Vand,* for ON. *vatn,* Sw. *vatten.* The form *vann* is still used in Scandia for *vatten.*

(33) *ćita* 'pail, bucket'; also written *kjita*; cogn. with Eng. *kit* 'tub' and probably also with *kettle. Kit* in the sense 'outfit' is still used in the Amer. slang phrase: *whole kit and boodle* (*caboodle,* a cowboy word from Span. *capital* stock, property).

(34) *dūnte* = *du inte* thou not.

(35) *lāń* = *lā-iń* 'let him' = D. *lad'en* (pron. colloquially *la-en* = *lad ham*).

(36) Note the direct oration peculiar to primitive idioms.

Grammatical Sketch.

Article and Noun.

The grammar of B. is much more complicated than that of either Danish or Swedish, the chief reason being that in B. a distinction is made between the masc. and fem. genders, whereas in both Danish and Swedish this distinction has long passed away, the two genders having completely coalesced in Danish and also in all Swedish except the most antiquated style, which, even in church writings, is rapidly going out of use. Thus, it is unusual to-day to find the masc. *e*-form of the def. adjective as distinct from the fem. and neuter form. All Swedes say and write at the present day *min kära vän* 'my dear friend' (masc.) instead of the more correct older form *kare* for masc., while *kära* was used for fem. and neuter definites. A similar blending of the masc. and fem. genders has all but taken place in modern Dutch, where one says *ik zie de*

man 'I see the man,' instead of *ik zie den man*, and this, in spite of the efforts of the purist school, who are striving to retain the older form, at least in writing. B. is one of the few living Scandinavian idioms, not excepting Icelandic and Faeroese, in which both the definite and indefinite articles distinguish the masc. from the fem. It is true that in Icel. and Faeroese a distinction is still made in writing; viz., masc. *hinn*, fem. *hin* 'the,' but the pronunciation is the same for both genders. B. has both an indef. and def. article different for all three genders; viz., indef. *iń mań* 'a man'; *en kånna* 'a woman'; *ed hūz* 'a house,' a peculiarity paralleled chiefly in the Juttish of Vensyssel and to some extent on Fyen. In B., the def. article is *daeń, daen, dedh* for the prefixed form, which is, however, not so common as the suffixed definite, whose forms are masc. *-ń, -iń*; fem. *-n, en*; neut. *-d, -ed*; pl. *-na* for masc. and fem., and *-n, -en* for neuters. It will be observed that the masc. sg. is distinguished by the palatal *-ń*.

The following paradigm will illustrate the method of using the def. suffixes:

STRONG NOUNS.

ārm, mō, dā, sag, brō, ȯ, hūz, trǣ

Masc.	*ārmiń* 'the arm'	*mōiń* 'the windrow'	*dāń* 'the day'
Pl.	*ārmana*	*mōana*	*dāna*
Fem.	*sāgen* 'the affair'	*brōen* 'the bridge'	*on* 'the island'
Pl.	*sāgarna*	*brōarna*	*orna*
Neut.	*hūzed* 'the house'	*trǣed* 'the tree'	
Pl.	*hūzen*	*trǣen*	

WEAK NOUNS.

tīma, bårrjara, kånge, ȯga, ćaerća, fårstå(e)lse, ȯra, aebbla, rǣje

Masc.	*tīmań* 'the hour'	*bårrjariń* 'the citizen'	*kångiń* 'the king'
Pl.	*tīmana*	*bårrjarna*	*kångarna*
Fem.	*ogan* 'the week'	*ćaerćan* 'the church'	*fårstå(e)lsen* 'the understanding'
Pl.	*ogarna*	*ćaerćarna*	*fårstå(e)lsarna*
Neut.	*orad* 'the ear'	*aebblad* 'the apple'	*rǣjed* 'the kingdom'
Pl.	*orn*	*aebblen*	*rǣjen*

There are certain rules indicating variations of the connecting vowel between the noun and the def. articles, which need not be entered into here.

It should be noted, however, that *horra* or *hårra* 'boy' makes its def. *hårriń* 'the boy'; pl. *hårrana* 'the boys,' like *bårrjara* (see above). Like *öra* 'ear,' only the neut. *åjja* 'eye' occurs, pl. *åjjen* 'the eyes,' but def. sg. *åjjed* 'the eye,' probably a Danism resembling D. *öiet* 'the eye' (pr. *ojedh*), for the earlier B. *åjjad*.

It will be observed that in these forms, as well as in the following examples of inflection, B. resembles modern Sw. more than the standard D. This is especially noticeable in the neuters; cf. Sw. *öra-t* 'the ear,' pl. *oron-en*, in which word there is no distinction in B. between the indef. and def. pl., *i.e.*, both = *örn*. The same principle applies to the Sw. *apple* 'apple,' pl. *äpplen*, but def. pl. *äpplena*. On the other hand, B. *hūz* 'house,' pl. *hūz*; but def. pl. *huzen* 'the houses' is exactly like the Sw.: *hus*, pl. *hus*; def. pl. *husen*.

Noun Inflection.

So far as nouns and adjectives are concerned there is no case inflection such as appears in Faeroese or Icelandic, but the system of forming the plural (indef.) of nouns should be briefly illustrated. The resemblance is closest to the Sw., although the B. forms themselves are not directly derived from Sw., but are a normal development of OD.

It will be observed from the following examples (*a*) that the strong and weak masc. groups incline to the pure *a*-plural, with the exception of a few instances; (*b*) that the strong and weak feminines both incline to the *-er* (*-r*)-ending, and (*c*) that the strong neuters have generally no pl. ending, while the weak neuters incline to *-n*, or also have no ending.

STRONG NOUNS.

Masc.	*ārm* 'arm'	*āwsel* 'axle'	*mō* 'windrow'	*dā* 'day'	*gaest* 'guest'
Pl.	*ārma*	*āwsla*	*mōa*	*dā*	*gaester*
Fem.	*sāg* 'affair'	*āwsel* 'shoulder'	*brō* 'bridge'	*o* 'island'	*nál* 'needle'
Pl.	*sāger*	*āwsler*	*brōer*	*or*	*nala*
Neut.	*hūz* 'house'		*trǣe* 'tree'		*harred* 'district'
Pl.	*hūz*		*trǣe*		*harreder*

WEAK NOUNS.

Masc.	*tīma* 'hour'	*bårrjara* 'citizen'	*kånge* 'king'
Pl.	*tīma*	*bårrjara*	*kånger*
Fem.	*òga* 'week'	*ćaerća* (*ćaerka*) 'church'	*fårstå(e)lse* 'understanding'
Pl.	*oger*	*ćaerćer* (*ćaerker*)	*fårstå(e)lser*
Neut.	*ora* 'ear'	*aebbla* 'apple'	*rǣje* 'kingdom'
Pl.	*öra*	*aebbla*	*rǣje*

The above divisions are easily understood. It will be observed that the class represented by *dā* 'day' (masc.) does not change for the pl. It is a model for such words as *stā* 'place' = ON. '*stādhr*.' Only practice can teach the learner when nouns ending in a vowel take the *-a* in the pl. like *mō-a*. In the strong fem. nouns, the class represented by *nål* 'needle' is very small. *Saeń* 'bed' (f.) belongs to it, as does *aeń* 'meadow.' The pl. of *pibel* 'little girl' is *pibla*, as the second component element is *baell* 'child,' pl. *baella*. Of the strong neuters, some neuters of foreign origin belong to the *harred*-class (pl. *-er*), as *bēst* 'beast,' pl. *bēster*; *insekt*, pl. *insekter*, etc. Of the weak masculines, the *tīma*-class represents the old *i*-masculines, as ON. *timi* 'hour.' Many nouns belong to the *oga-ćaerća*-class. The weak neuters comprise (*a*) a small group in *-a* = the ON. neuters in *-a*; (*b*) some neuters in *-e* = the ON. neuters in *i*. Note that for *åjja* 'eye' = ON. *auga*, one of the few B. nouns inflected like *öra*, there is an old B. form *īva* 'eye,' pl. *īven*. *Jārta* 'heart' has pl. *jārta*. A very large class is inflected like *aebble* and *rǣje*. The ending *u* is seen in *vińńu* 'window' (sg. and pl.) and *varru* 'business' (cf. Icel. *verslun*), but B. *hönnu* 'honey' is used only in the sg. = OD. *hunugh*.

Adjectives.

The indef. adjective in B., like that in ON. and modern Icel. and Faeroese, distinguishes between all three genders; viz., masc. *gōer* 'good,' fem. *gō*, neut. *gått*, pl. *gōa* (all genders)

fattier 'poor'	*fatti*	*fattit*, pl. *fattia*
vīder 'wide'	*vīd*	*vitt* (= *vidt*), pl. *vīda*

The rule is that the indef. must end in *-er* in the masc., the fem. has no ending, while the neuter adds *-t* which frequently modifies the preceding syllable. The indef. pl. ends in *-a* for all genders.

No such distinction is made in the def. adjectival form followed by the definite noun:

Masc.	*goa* '(the) good'	pl. *goe*
Fem.		
Neut.		
	fattia '(the) poor'	pl. *fattie*
	vīda '(the) wide'	*vide*

The def. sg. of all genders ends in *-a* (there is no masc. def. *-e*, as in old Sw.), while the pl. ending for all genders is *-e*, a distinctive Bornholmism based on D. *-e*, def. pl. and sg.

Adjectives form their comparative by *-ara* (more often now *-ere*, following D.) and the superlative by *-ast*, def. *-esta*. Thus *brūner* 'brown,' *brūnara*, *brūnast*; but def., *brūnesta*; pl. *brūneste*. The same irregularities appear as in the other Scandinavian idioms; *långer* 'long'—*laengre*—*laeńst*; *gōer* 'good'—*b̄aere*—*baest*, etc. The superlative has no differentiation for gender and number in the indef., but has both, as just shown, for the definite.

Numerals.

The numerals up to ten cardinals and ordinals are as follows:

Masc.	Fem.	Neut.	Danish			Danish
iń	*en*	*ed*	*en*, *et*	'one'	*fårsta* (*fåsta*) 'first'	*forst*(*e*)
to			*to*	'two'	*ānra* (pl. *āndra*)	*anden* (pl. *andre*)
trē			*tre*	'three'	*trede*	*tredje*
fīr(*a*)			*fire*	'four'	*fjaere*	*fjerde*
faem			*fem*	'five'	*faemte*	*femte*
sajs	'six'		*seks*		*śaete*	*sjette*
śu	'seven'		*syv*		*śuene*	*syvende*
åt(*a*)	'eight'		*otte*		*åtene*	*ottende*
ni	'nine'		*ni*		*niene*	*niende*
ti	'ten'		*ti*		*tiene*	*tiende*

It should be observed that the numerals 'fifty' to 'ninety,' incl., are not formed as in Sw. and Norse *faemti*, *seksti*, etc., but as in D., by the addition of *hall-* and the suffix *sensćive*; viz., *halltrósensćive* 'fifty,' D. *halvtredsindstyve*, etc. This method of reckoning is peculiarly Danish, and resembles the Celtic system, still used in Welsh, and appearing in French.

PRONOUNS.

B.		D.	Sw.	B.		D.	Sw.
jā	'I'	*jeg*	*jag*	*hań*	'he'	*han*	*han*
māj	'me'	*mig* (pr. *maj*)	*mig* (pr. *mej*)	*hannöm*	'him'	*ham*	*honom*
				hon	'she'	*hun*	*hon*
vī, vi	'we'	*vi*	*vi*	*haeńe*	'her'	*hende*	*henne*
vås	'us'	*os*	*oss*				
ī, nī	'you'	*i*	*i*	*di*	'they'	*de*	*de*
jaerr, jarr	'you' (acc.)	*jēr*	*Eder* (*er*)	*dåm*	'them'	*dem*	*dem*

The possessives are respectively *mīń*, *mīn*, *met* 'my'; pl. *mīna*; *dīń*, *dīn*, *det*; pl. *dīna*; refl. *sīń*, *sīn*, *set*; pl. *sīna*, declined like indef. adjectives; *hans* 'his'; *haeńes* 'her' indecl.; *vår* (*våres* and rare, *våresa*), neut. *vårt*; pl. *våra*; *jaer*, neut. *jaert* 'your'; pl. *jaera*, declined like adjectives and *dårre* 'their,' indecl. The relatives are simply the indeclinable *daer* (nom. only) and *såm* 'who, which, what.' The interr. is *vekkiń*, *vekken*, *vekked*; pl. *vekkene*, a pronominal indef. adjective.

VERBS.

The B. verb, like that of the other Scandinavian dialects, has only two pure tenses; viz., the present and preterite, making the future and past definite by means of auxiliaries. B., unlike D., distinguishes between the sg. and pl. of the pres. tense by omitting the *-r* of the sg. and in most instances by using *-a* in the pl. In the pret., as in Sw., B. sometimes has a distinctive pl. form, which, as in colloquial Sw., is dying out in ordinary usage.

The following examples of both strong and weak verbs will suffice to show the formation: *å bińa* 'to bind'; *jā bińer* 'I bind'; *vī bińa* 'we bind'; *jā bānt* 'I bound,' *vī bońe* 'we bound'; *jā hār bońed* 'I have bound' = D. *at binde, jeg binder, vi binder, jeg bandt, vi bandt*; *jeg har bundet*; Sw. *att binda, jag binder, vi binda*; *jag band, vi bundum*; *jag har bundit*. *Ablaut* is as common in B. as in its sister idioms; thus, *ǵēr* 'gives'; *gā* 'gave,' but *ǵēd* 'given'; *fār* 'drives'; *fōr* 'drove'; *fōred* 'driven,' etc. The verb 'to go' requires special mention; *å gå* 'to go'; *jā går, vī gå*; *jā ǵiće, vī ǵeće*; *jā e gåd*. In Gudhjem, this verb still has the half weak form *gåńńa*; *gåńńar, gåńńa*; *gåńńada*; *gångiń*.

The weak verbs form the pret. by *-de*, *-t* and the past partc. by masc. *-der*; fem. *-d*, neut. *-t*, which neuter form is used with the auxiliary *ha* 'have' to form the pure past definite. Thus, *å rīa*

'to ride'; *jā rīer, vī rīa; jā, vī rīde* ('rode'); *jā hār rīt; å taella* 'to count'; *jā taeller, vī taella; jā, vī talde, jā hār tālt; å vīla* 'to rest'; *jā vīler, vī vīla; jā, vī vīlada; jā hār vīlad.*

The auxiliaries are *końńa* 'to be able'; *jā kań* 'I can'; *vī końe* 'we can'; *jā, vī końńe* 'we could'; *jā hār końńad; jā skā* 'I shall'; *skol'l'e* 'should'; past partc. *skolad; vel'l'a* 'to will'; *jā vel*, pl. *vī vella; jā, vī vel'l'a* 'would'; *jā hār vellad* 'I have wished.' The verb 'to have' is *å hāva, jā hār, vī hā, jā, vī hāde, jā hār hād.*

The middle passive in *-s* appears, as in D. and Sw., as *saettas* 'to be put' and the *-s* is suffixed to all the tenses, including the past partc. *satts.* The direct use of the *-s* has disappeared in D. with the past participle, where the *-s* is suffixed as a rule to the pret. *-de* and the tense construed with the verb *vaere* 'to be' as *det er lykkedes*; but Sw. *det har lyckats* 'it has succeeded.'

Finally, the following forms should be noted: *gak*, imper. of *gå* 'go'; *ēstu* 'art thou'; *vāstu* 'wert thou'; *såstu* 'sawest thou'; *tāwstu* 'tookst thou,' etc. The old gerundive in *-s* also occurs in B. *drikkenes* = D. *drikkende* 'drinking,' etc.

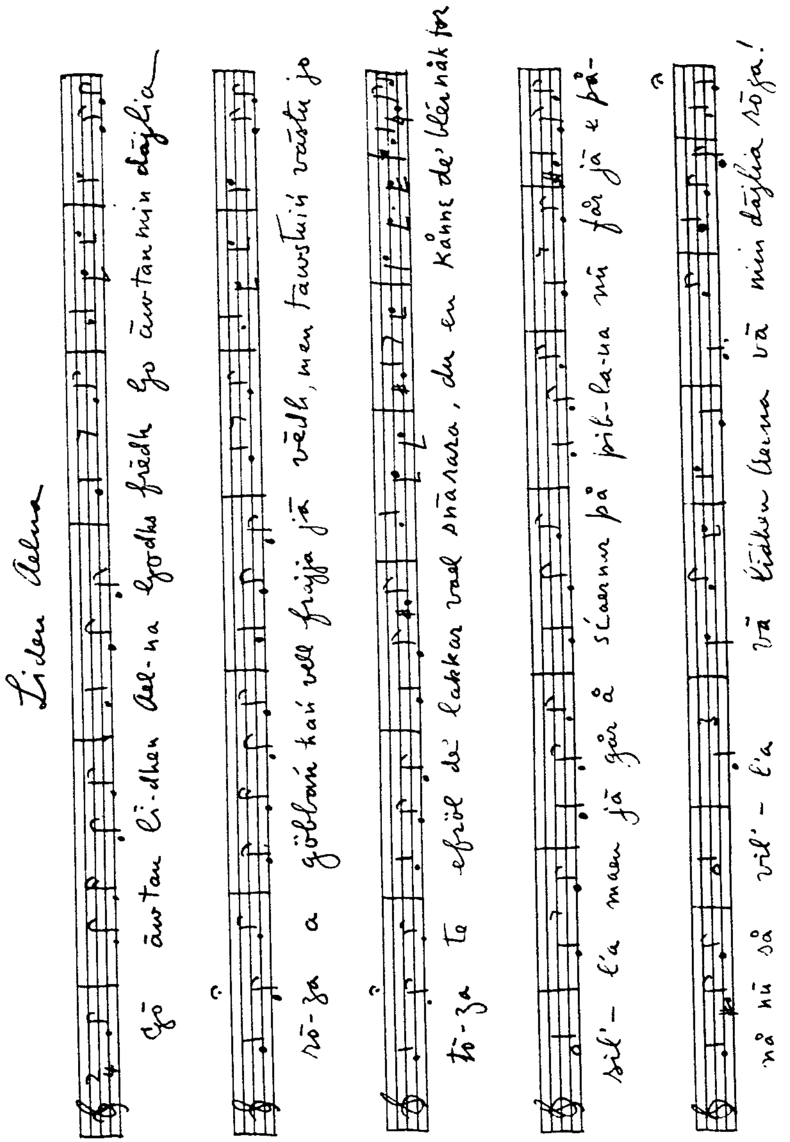
Music.

The Bornholmers have preserved a wealth of folk-songs, many of them of real value and excelling those of any part of Denmark in beauty, although none of the D. folk-songs are equal to the Sw. in pure melody and musical form. The Danes have of late years begun to discard their old folk-music for modern European productions and, in their desire not to be "Scandinavian," are inclined to look upon anything essentially old Danish as "vulgar" and belonging to the "servant" classes. This, however, is not the case on Bornholm, where a genuine pride is felt in the old airs.

The following specimen, giving the melody [10] of the song recorded above (II.), is a very characteristic Bornholm air. It will be noted that the old Scandinavian influence lingers in the musical phraseology, a phenomenon which is apparent in many of the island songs, where, of course, the Scandian influence predominates, although some few of the refrain-songs show German contact.

Legation of the United States of America,
Copenhagen.

[10] *Viser i Bornholmsk Mundart*, p. 134 (n. 6).



ARE THE VARIOUS RACES OF MAN POTENTIALLY EQUAL?

By H. N. HALL.

(*Read April 25, 1924.*)

In the absence of accurate special means for the appraisal of intelligence and character, it is permissible to apply the test of history to the consideration of the achievements of a race and from an examination of its past performances to predict its probable future.

In order to answer the question which has been proposed, we may restrict our attention to one group of people relatively homogeneous in physical constitution and appearance, that is to one race. If we find that the culture of such a group is likewise homogeneous and if study of this culture reveals a certain type of spiritual weakness which has kept the race backward throughout its history, we are of course justified in saying that this race is not the equal of the race or races whose mental vigor has been responsible for the building up of what we know as civilization. If this kind of inferiority is incurable and so, we may infer, congenital, we must of course also deny that the race in question is potentially as well as actually the equal of the civilized peoples.

I believe that a study of the culture of the negro and of the history of his contacts with white civilization justifies this conclusion with regard to him.

The most instructive of negro institutions for the purposes of our problem is that of the kingship or chieftainship in Africa.

Autocracy is characteristic of negro government and that in the highest degree where government is most highly organized. And on all sides in Africa we find that certain peculiar characters accompany the institution. Either the ruler is a magician or he is in some sense divine.

In the region of the upper Nile, among such tribes as the Bari and the Latuka, the magicians, whose principal function is that

of rain-making, are the chiefs. "Rain is the one thing which matters to the people in those districts, for if it does not come down at the right time, this means" that their animals and their crops, their only resources, are destroyed. These chiefs, the rain-makers, have various ways of producing rain. One may have a collection of rain-stones, such as rock-crystals and amethysts. These he plunges into water, and taking a cane, beckons with it to the clouds, accompanying his gestures with an incantation. Another will pour water upon the entrails of a sheep or goat placed in the hollow of a stone, and then sprinkle the water towards the sky. Among the Dinka, the rainmaker chiefs are believed to be animated by the spirits of former great rainmakers. The king of the Shilluk on the White Nile reincarnates the spirit of the semi-divine founder of the dynasty who brought the tribe into its present territory. Cattle are dedicated to the shrines of the kings and sacrifices are offered there as well as at the shrines of their great forerunner. The regard, reverence, or worship which the Shilluk pay to their king is due to the fact that the spirit which is reincarnated in him is responsible for the health and increase of their cattle and crops.

In East Africa we find a similar state of affairs. In Uganda, the sympathetic relation that is believed to exist between the vital forces of nature and those of the king is again shown in the tending of certain trees planted near the king's residence. "It was believed that as they grew and flourished so the king's life and powers would increase." But the confidence that the royal or divine power was so closely bound up with the forces of nature that the latter were controlled by it is more directly seen in Uganda from an institution which at intervals temporarily superseded the kingship. There was a god who lived in Lake Victoria who sometimes took up his abode in a priest or priestess. When this incarnation took place the person in whom the god was now incarnate became for the time being the supreme power in the land, not only in matters of faith and ritual but in questions of war and politics. He was consulted as an oracle; not only could he inflict or cure disease, but, above all, he could withhold rain and cause famine.

In Mombasa the king was able to produce rain. Further south, we hear from a Portuguese historian of the Zimba or Muzimba who

regarded their king as a divinity. If it rained when he did not wish it or if it was too hot, he shot arrows at the sky for not obeying him. This is no doubt a half understood statement of the powers of a royal rainmaker; on the opposite coast, the king of Loango brought rain by shooting an arrow up into the air.

In South Africa, much the same conditions are found. Sorcerers are held in great esteem, especially for their rainmaking powers. If they are not always rulers, they are held in such regard as to be formidable rivals to the chiefs. Chaka, the famous Zulu tyrant of a century ago, declared that he was the only magician in his country. He would tolerate no rival who laid claim to magical power.

In West Africa, in Lower Guinea, the fetish ruler Kukulu regulated the winds and storms. In Togoland, a fetish priest who is the real ruler of his part of the country had charge of the fetish Bagba. This fetish sent or withheld the rain and was lord also of the winds. The King of Loango, as we have seen, had a similar power over the elements. In the parts of West Africa further north, where the purer negro as distinguished from the Bantu culture obtains, we find sometimes a division of power between a fetish king and a civil king, although the former is really supreme, and here again he controls the weather. Where, as in Dahomey or Ashanti or Benin, powerful states were formed, the tendency has been to unite the fetish and civil powers in a single ruler.

These instances serve to show the wide prevalence in negro Africa of this attitude towards chiefs and kings. It indicates clearly enough, I think, a willing subservience of the negroes towards their rulers not through any consciousness of political necessity or expediency but because of an inveterate tendency in the negro mind to seek safety in dependence on a superior being, to shift the responsibility for coping with difficulty and danger to some power or authority outside himself—even in the ordinary crises which arise in the daily course of events. It is obviously due to weakness of will and inability to persist in the most necessary and usual undertakings without leaning on the support of a superior.

These ruling magicians and divine or semi-divine kings, who are a response to the demand for some extraordinary power to deal with the unexpected or the merely difficult or dangerous, are

themselves often curbed with all sorts of limitations and surrounded with taboos. This is evidently due to the same factor in the negro disposition. The chief is, after all, at any rate to some extent, of the same human nature as his subjects and hence to some degree shares in the disabilities which make them a prey to the caprices of the mysterious forces of nature. So he must be hedged about with rules of behavior which insure the maintenance of his sympathetic relation with those forces. It is simply a case of making assurance of safety doubly sure. The regulations not only restrict the chief's liberty but sometimes even involve his life. So long as the magically or divinely controlled relations with the all-powerful forces is maintained, the individual controller is often of secondary importance.

One of the conditions on which a ruler of this kind holds power is that he must show no symptoms of bodily weakness or decay. It is the kind of analogy on which the assumptions of sympathetic magic are based. If the chief's strength diminishes, if he becomes ill or senile, this has a similar effect on the forces with which he is in sympathetic relation or which, embodied in him, control the increase of the crops and herds. The Shilluk king spoken of just now was put to death when he showed signs of ill health or of failing strength. No Dinka rainmaker was allowed to die a natural death of illness or old age; he was put to death when his physical powers showed signs of flagging, lest the tribe should suffer from disease and famine and the herds fail to yield their increase. Among the East Central African neighbors of the Baganda, the Banyoro, if the king fell seriously ill or began to fail through old age, he had to commit suicide by taking poison; if he faltered in this duty, his wife must administer the dose. On the upper Congo the king of Kibanga was strangled under similar circumstances. Among the Hausa in Northern Nigeria an ailing or infirm king was throttled by a special official known as the killer of the elephant. There is reason to believe that a similar custom once existed among the Zulus. Even a slight bodily blemish, such as the loss of a tooth, was a sufficient pretext for insisting that the Kaffir king of Sofala must take poison. That even in Africa the custom may be pushed to the point of extinction through its own absurdity is shown by the case of the kingdom of Kabinda at the mouth of the Congo. The rule there was that the chief who became its

ruler was always killed on the night after his induction into office. For a long time there have been no claimants to the throne of Kabinda.

Yet at Gatri, on the Benue River, a tributary of the Niger, where the number of years a king was to reign was settled by the electors, although the candidate knew that after a limited time he would be made drunk on guinea-corn beer and then speared, such is the inertia of negro custom and such the fatalism which accompanies the typical negro disposition, that there is no record of the royal stool standing vacant.

In ancient times the Ethiopian kings of Meroë were worshipped as gods. Yet their lives were at the mercy of the priests, who, when it pleased them, used to send a messenger to the king to say that an oracle of the gods had decreed his death. Ergamenes, a contemporary of Ptolemy IV, was the first king to disobey this order. When the message arrived he took a body of soldiers to the temple and put the priests to the sword. Ergamenes had received a Greek education which must have endowed him with a small amount of resolution necessary for so elementary an effort at self-preservation. And his example does not seem to have had a lasting effect. In some tribes of Fazokl, in the very same region, down to modern times the king had to administer justice daily under a certain tree. If from sickness or any other cause he failed to discharge this duty for three successive days, he was hanged on that tree in a noose which contained two razors so arranged that when the noose was drawn tight by the weight of his body the razors cut his throat. The people of Meroë have from the time of Herodotus been of mixed race, with the negro element always growing stronger down to the present time.

Of other restrictions on the liberty of action of persons of royal or chiefly rank in negro Africa it is necessary to mention only a few. The fetish king Kukulu, of whom we have heard before, lived alone in a wood; he must not leave his house; he must not touch a woman; he must not even leave his chair; he must sleep in it sitting, because if he lay down no wind would blow and navigation would be stopped. Among the Evhe-speaking peoples of the Slave Coast, the king, who was also the high priest, could leave his house only by night. Only his representative to the people, the so-called "visible king," together

with the three chosen elders, might speak to him and then only when seated upon an ox-hide with their backs turned to him. He was not allowed to see any European or to look at the sea. The kings of Dahomey and of Loango were subject to the same prohibition with regard to the sea. In Loango as in Uganda, the king must not be seen eating, on pain of death to the beholder. As a general rule the purpose of these restrictions, like those which caused great chiefs to be put to death, is in some way or other to preserve unimpaired the powers of the chief which direct or sympathetically affect the forces of nature which it is desired to control.

It is not claimed, of course, that this kind of institution is peculiar to the negro. The institution of the divine chief with his taboos is known also, for example, especially in Fiji and Polynesia. Something not unlike it may even have obtained at one time among our own ancestors. But among the people from whom our civilization in this country is chiefly descended, whatever divinity may once have hedged a king was, as far back as we can trace the institution, counterbalanced by checks and restrictions which had a political rather than a religious bearing. The system had in it the germ which made possible the further development of individual and social freedom, as distinct from a system like that of the negro, which by its very nature was a closed one—incapable of further useful development. The restrictions binding the negro chief and his people in their relations to him could only perpetuate superstition and submission to blind forces. It is true that restrictions of a political kind on the power of the chief were also present. The foreign and domestic policies of negro states were often subject to control by a council of elders. But this only tended to throw power into the hands of an oligarchy who could use the supernatural or magical qualities attributed to the king and to themselves for the maintenance of their own power. In other cases this political limitation of the supreme power was made futile by the fact that the ruler by magical or divine right had absolute power of life and death over all his subjects and could also confiscate their property.

It has been said that the power of inhibition involved in the observance of innumerable taboos binding not only the chiefs but their subjects also is not consistent with feebleness of will and the conse-

quent lack of persistence in effort. Such taboos commonly prohibited various kinds of food and also on certain occasions any contact with women. In view of the well-known uxoriousness of the negro, it is claimed that abstinence of the second kind esecially shows that he could exert considerable will power. But it has to be remembered that such taboos were enforced by what was for the negro a very real terror. As between the penalty for the infraction of a taboo, which was often death, and a merely temporary abstinence, there was not, one would think, much difficulty of choice.

The history of the negro in the New World shows that even where he has been removed from the surroundings in which his own institutions were developed the mental disposition out of which those institutions grew has persisted unchanged in essentials. He still leans upon forces outside of himself. The fetishism of which the institutions which we have been considering are the most highly developed expression still persists. In the British colonies in the New World it is still necessary to keep in the laws severe penalties against obeahmen, whose influence remains strong. This in spite of the fact that the negroes have been subjected to the influence of European civilization for three hundred years, and that during a great part of that time ministers of numerous Christian sects have worked diligently to root out superstition. In Haiti for more than a century the negro has had the opportunity to work out his own salvation under leaders of his own race; and it is precisely in Haiti that we find voodooism and political oppression and corruption dividing the field of authority between them. In the United States and elsewhere on this side of the Atlantic such limited progress as the negro has made is due to white philanthropists and to leaders of mixed race. These are his external superior forces and he knows it, and he leans on them.

So far as the evidence from history is concerned the peculiar mental disposition of the negro is unchanged, and it seems unchangeable. This mental disposition keeps him backward and we have no reason to suppose that in comparison with the white man he will ever be anything else.

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ARE THE RACES POTENTIALLY EQUAL?

By ALEXANDER GOLDENWEISER

(*Read April 25, 1924.*)

Mr. Hall has just regaled us with delightful bits of negro magic. There is no denying the interest and quaintness of the data he has presented. But what is the bearing of this folk-loristic panorama on the question of racial equality or ability?

As an anthropologist Mr. Hall need not be told that even in the very field of magic he has so thoroughly scanned, modern man can not by any means boast of a clean slate. We have heard of the supernatural powers attributed to negro kings. But how about the kings of Europe—today (or shall we say yesterday)? When some two generations ago the imperial train carrying Alexander III. and the royal family was derailed with tragic consequences, but their majesties suffered no injury, the event was hailed throughout Russia as a manifestation of Divine Providence. Needless to say, the ruler of All the Russias lost no time making capital of this special sign of supernatural favor. Or shall I remind you of William II. who made no secret of his belief that his ideas and acts were directly inspired by the Creator? And in this faith he did not stand alone.

With us, moreover, as with the African negroes, supernaturalism is not by any means restricted to royalty. Women no longer believe that the eating of a twin banana will result in a similar multiplication of offspring, but many of them, when about to become mothers, still frequent concerts and libraries in the fond hope and expectation of thus imparting to their issue the qualities of musicianship and scholarliness. We may not see as many visions as was man's wont in earlier days, or when we do, discount them; but we still dream dreams, and many there are among us who do not shrink from ascribing to dreams the significance of omens and prognostications. Charms and amulets, mysteriously acting stones and the evil eye, symbolic numbers, lucky and unlucky days, mental action at a dis-

tance (and this is the very kernel of magic!)—all these are still with us. . . .

But let us waive this issue and glance at the problem of racial comparability in somewhat more systematic fashion.

First, then, comes man physical or biological. Are the physical differences between the races such as to permit a grading into a progressive series from the animal upward? The answer is a decisive *no*. When white man is examined as an integral specimen, his pale skin, his refinement of feature, his harmoniously symmetrical development and, last but not least, his æsthetic appeal to *our* senses, seem to prejudge the case at once in his favor. Surely no other race has reached a physical development so remote from that of the animal and otherwise so satisfactory, both anatomically and æsthetically! But if abandoning this impressionistic approach, one examines man feature by feature, the conclusion can hardly be the same.

Take, for instance, the red external lips, a trait distinctly human, for animals, even the highest, are devoid of it. Now no one can deny that in this particular feature the Negro race has traveled far ahead of the others and therefore furthest away from its animal prototype. Or, take hairiness of the face and of the body generally. From the standpoint of this feature, the Negro, Mongol and American Indian would justly claim a certificate of advanced humanity, for their hairiness, a trait characteristic of most animals, is but slight. The Australian, on the other hand, is in comparison quite animalistic, for he is very hairy indeed. In this, however, he is like one other race, namely white man, who is as hairy as the Australian.

And so on with other features. There is no possibility of a serial grading from animal to man as far as the actually existing races are concerned. They must be regarded as specialized developments from the animal, proceeding in different directions.

Then, there is man neurological and psychological. How does the case stand here? It is, of course, obvious that the general development of higher animals culminating in man, is accompanied by a progressive increase in the relative weight and size of the nervous system and in particular of the brain. But when the level of humanity is once reached, the case is no longer so simple.

Many efforts have been made to prove that races other than the white are, on the whole, characterized by smaller and lighter brains. In the light of more careful study this position can no longer be sustained. In the only instance where sufficient data for a definitive conclusion are available, that of the Negro and the white, the situation is about as follows: the majority of negro and white brains vary between the same limits of size and weight. However, a small number of white brains seems to occur which are heavier and larger than any normal negro brains and an equally small number of the latter which are smaller and lighter than any normal white man's brains. At first blush this seems impressive, but heed must be paid to an important complication. Do brain size and weight stand in a definite ratio to what is called intelligence? The popular view to the contrary notwithstanding, this question must be answered in the negative. It seems that within the limits of variation of the human species practically any degree of intelligence is compatible with any normal brain size and weight. In other words, speaking of the white race alone, a thousand intellectually distinguished individuals would in their brain size and weight be comparable to a thousand average individuals. It follows that no conclusion inimical to the negro race can be based on the above noted slight discrepancy.

There is, of course, the further question of brain convolutions which are in some way related to brain organization and mental performance. But this aspect of the subject is so replete with questions asked and unanswered, that no conclusion can be hazarded by the unprejudiced.

Then come the sense reactions and psychological processes generally. In this realm the notion is still prevalent that races other than the white are distinguished by sensory acuity but by a relatively inferior development of the so-called higher mental faculties. Both characteristics are, of course, interpreted in a sense inimical to the non-white races. To put it bluntly: they can not *think* as well as white man does but they can hear, smell and see better, just as some animals do.

But all this belongs to opinion, not to balanced objective judgment. Neither psychological tests nor our general experience with non-white races, when critically evaluated, reveal any superiority of

these races to white man in sensory acuity. It is true that in primitive conditions man lives in close contact with nature and that in this setting he often learns to make better use of his senses than does white man in his highly artificial civilization. But all this is a matter of habituation through practice. The Australian bushman would fare no better on Broadway than does a white man in the jungle!

As to other psychological traits, the consensus of opinion of unprejudiced mankind, among whom are missionaries, travelers, and scientists, is to the effect that for better or worse, man's mind works about the same the world over. Also, is it not fairly obvious that the psychological characteristics usually attributed to the races are once more of the acquired, not of the congenital, variety? The stolidity of the Indian, the reserve of the Mongol, the emotionalism of the Negro, are not any more inherent qualities than is the vindictiveness of the Italian, the logic of the French, the pedantry of the German, the sentimentality of the Russian, the stupidity of the English, or the frankness of the American. Each and all, these traits are cultural, acquired through education and subject to change when social environment changes.

I do not want to be understood as holding that the psychological identity of races is either proved or likely to be so. The contrary is, in fact, more probable. That the great physical differentiation of the races should not have been accompanied by some corresponding psychological differentiation, is hard to suppose. Some day when we are better informed about these matters we may find that certain psychological differentia do obtain between the races. On the other hand, it would be gratuitous to assume that such differential racial traits would prove classifiable as higher and lower in the range of mental aptitudes.

I realize that all this may impress you as unduly detached and academic. What do we care, I hear you say, about lips and hairiness, heavy and light brains, sensory acuity and psychological characteristics! Our concern is with civilization and history. White man's historic career and his alone was steadily progressive, he and none but he has achieved a genuine civilization beside which the cultural vagaries of other races dwindle into insignificance.

This sounds good and to a degree carries conviction—until ex-

amined in a dispassionate mood. For is it so certain, after all, that our civilization, point by point, is superior to all others?

In certain domains, yes. We know more—this is science. We have learned to apply knowledge to life—this is applied science. Also, we are able, at times, to control thought by knowledge—this is scientific theory and philosophy. In these domains, indubitably, we reign supreme. But can as much be said about our religion, ethics, art or socio-political organization? No sooner is this issue broached than it becomes at once apparent that no conclusion can be reached here without postulating some standards of reference. This, as a rule, is done unconsciously when our own standards are taken for granted and the achievements of other civilizations or races are envisaged in the light of these standards. But then, they must be granted the right to proceed similarly, which, indeed, they often do with results entirely satisfactory to themselves.

Is monotheism, for example, superior to pantheism or to an universal animism? It all depends. If conformity to a world view is accepted as the standard, then, from the angle of a pluralistic philosophy, animism may well appear as an ideal form of supernaturalism. For does it not spiritualize and deify the particular? Monism, in comparison, would pass as a product of a futile and vacuous abstractionism. Or take morality: if to be moral is to *live* a code of behavior not merely to profess one, then most primitive folk as well as some of the less remote civilizations would easily outclass us in an estimate of moral stamina. As to art, a rattle from the Northwest Coast of America or a Polynesian club are, within their range, as perfect examplars of art as a painting from the brush of Raphael, a statue of Rodin's or a Gothic cathedral.

It is also useful to remember that modern civilization, in its most distinctive elements—that is, knowledge and its theoretical and practical application—dates, as it were, of yesterday. In positive knowledge we are almost as superior to the ancient Greeks and Romans as we are to the African negroes and the Australian bushmen. In the mechanical aspects of living—means of production, transportation, communication—there is more difference between the man of today and one of the eighteenth century than there is betwen the latter and his forebears of the prairie and the forest.

In a fortunate moment of history a set of striking inventions resulted in unique changes in knowledge and achievement and, while these changes were not by any means wholly on the credit side of culture, they did in more than one way constitute an unprecedental advance.

But, to assume that such a forward push could not occur in any other race and culture than ours, would be siding with gratuitous assumption and prejudice rather than with probability.

As I am speaking, I am becoming painfully aware that some of you are suspecting me of making a rather naive assumption, the assumption namely that the problem of racial equality revolves about fact, knowledge, critical thinking. Before closing, permit me to defend myself against this unmerited suspicion. I am only too painfully conscious that this is not the case, that the question of race is a province for garbled facts, special pleading, prejudice, conceit, jealousy, and selfishness. To admit this is perhaps the first step toward a more rational and humane attitude.

Remember Japan! A generation ago the Japanese stood no higher in the estimation of their white brethren than the Chinese stand today. Then came the Russo-Japanese War. The Russian eagle was forced to eat dust at the feet of its yellow conquerors. And out of the ashes of the war Japan arose a full-fledged member of the family of nations. If today we still discriminate against the Japanese—and, to our shame be it said, we do—it is no longer on the ground of racial inferiority.

The lesson of this historic episode is well worth the learning. What happened once may happen again. To satisfy her imperialistic ambitions, France is at this time engaged in providing military training for vast numbers of her African subjects. These troops or those that will follow in their wake may not always remain satisfied to lay their bones on the battlefields of Europe in the interests of a foreign and selfish power. Some day they may become the nucleus of an Africa for Africans, the vanguard in the struggle of the black world for racial emancipation.

The fate that befell Russia at the hands of the yellow men of Asia may befall others when they are confronted with the black

legions of Africa. Then we shall recognize them. Then we shall grant them the right to world citizenship.

But must they wait so long? Must we? Remember there is a price to pay for the delay.

Might it not be wiser as well as safer to proceed more expeditiously?

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INHERITANCE BY TETRAD SIBS IN *SPHÆROCARPOS*.

BY CHARLES E. ALLEN.

(*Read April 23, 1921.*)

In *Sphærocarpos Donnellii* Aust., as in some other members of the same genus, the four spores formed by the division of a single spore mother cell are (certain strains excepted) regularly held together at maturity. If the spores of such a tetrad germinate, four haploid plants (gametophytes), and ultimately four clones, are produced whose genotypic constitutions result from the distribution of chromosomes in the course of the reduction divisions that formed the spore nuclei. *Sphærocarpos* thus offers a special opportunity for observing what is virtually the immediate result of the segregation of chromosomes in the reduction divisions.

To realize the advantage here presented, it is necessary to secure germination of a large proportion of the spores sown, and to grow the resultant clones to a size sufficient for the satisfactory determination of their characters. The genetic work thus far done with *Sphærocarpos* has encountered serious difficulties in the germination of spores and the growing of sporelings. Two sets of germinations, as reported in a previous paper (Allen, 1919), were sufficiently successful to give some information as to the inheritance of sex and of characters closely associated with sex. But in the study of the inheritance of other characters, the most extensive results, reports on some of which are now in press, have until very recently been obtained by sowing the spores or spore tetrads broadcast upon soil, and by later isolating the sporelings. By this method it may be learned what genotypes occur, and in approximately what proportions, among the offspring of a given mating—results comparable with those obtained in the genetic study of other organisms—but not what is the distribution of the different genotypes within a single tetrad.

During the last two years, however, the use of improved methods has resulted in a larger proportion of germination on the part of the

spores of isolated tetrads. While the results that can be reported at present are still comparatively meager, and while some statements as to the character of individual plants must still, as will appear, be made in tentative form, yet the facts at hand are sufficient to indicate the possibilities inherent in this method of analysis of inheritance. The sowings of spore tetrads whose results are here recorded were made in the autumn of 1922; the sporelings were separated during the early months of 1923 and transplanted to individual pots, where they developed into clones.

In these studies I have had the valued assistance of Mr. James A. Lounsbury, and the work has been carried on with the aid of an appropriation from the research fund of the University of Wisconsin.

Polyclady and Sex.

Among the divergent races of *Sphaerocarpos Donnellii* whose inheritance has been studied is one to which the term "polycladous" is applied. Polycladous plants of either sex are characterized by profuse and irregular branching, by marked variation in the form of the lateral lobes of the thallus, and by the occurrence of dorsal lobes and cilia. Antheridia are fewer than on typical male plants, sometimes lacking altogether on many, or even on all, of the plants of a clone. The antheridial involucres are, with rare exceptions, reduced in various ways, leaving the antheridia in consequence partly or entirely exposed. In vegetative structure, apart from their greater average size, polycladous female plants resemble the males. Only in very few instances have these female plants been observed to bear archegonia, and even when present archegonia have proved functionally sterile. The rare archegonia are enclosed in involucres more or less closely approximating those of typical form; elsewhere the polycladous females bear, in varying abundance, solid columnar upgrowths which seem to correspond to involucres—containing, however, no structures that suggest archegonia.

If a typical female is mated with a polycladous male, the gametophytic offspring (in the f_1 generation) consist of four classes: typical females, polycladous females, typical males, and polycladous males. The four classes occur, as previous studies have shown, in

at least approximately equal numbers. The sterility of polycladous females, of course, makes the reciprocal cross impossible.

Table I. gives the results of the germination of spores of 29 spore tetrads borne by two sporophytes (14 and 15 tetrads respectively) produced by the mating of typical females with polycladous males. The distribution of characters within the two families was so similar that, for economy of space, both are tabulated together. Only those clones are included in this classification, as well as in that of Table II., which grew to a large enough size to permit of a satisfactory determination of their character.

TABLE I.

TYPICAL FEMALE × POLYCLADOUS MALE.

2 Crosses.
20.337 × 21.148
21.85 × 20.266

	Female.	Male.	No. Tetrads.	
(*a*)	2 typical	. . .	2	
(*b*)	1 typical,	2 polycladous	1	
(*c*)	1 typical,	1 polycladous	1	
(*d*)	1 typical	. .	4	
(*e*)	. . . . .	2 polycladous	4	12
(*f*)	1 polycladous,	2 typical	1	
(*g*)	1 polycladous,	1 typical	4	
(*h*)	1 polycladous	. . . .	5	
(*i*)	. . . .	2 typical	2	
(*j*)	. . . . . .	1 typical	4	16
				28
	Exception.			
(*k*)	1 typical, 1 polycladous,	1 typical, 1 polycladous		1
				29

The facts summarized in Table I. make possible the formulation of certain general rules.

First, the facts harmonize with the expectation that, of the spores of a tetrad, two will carry the female, and two the male, potentiality. The results in this respect agree with those previously reported (Allen, 1919), as well as with the observation that in spore-formation two of the spores of a tetrad receive the X-chromosome character-

istic of females and two receive the Y-chromosome characteristic of males.

Second, the results indicate that two of the spores of each tetrad carry the potentiality for the production of the typical (non-polycladous), and two that for the production of the polycladous, character. In terms of current genetic conceptions, this implies that the appearance or non-appearance of the polycladous character is determined by the distribution of physical bases borne by the chromosomes of a single pair. In the course of the reduction divisions, a chromosome bearing the physical basis necessary to the appearance of polyclady passes to two of the four spores of each tetrad; the other two spores receive a different chromosome belonging to the same pair, and consequently the physical basis corresponding to non-polyclady.

Third, in all except the one instance noted as an "exception," the results are in harmony with a supposition that the four spores of a tetrad give rise either to two typical females and two polycladous males (classes *a–c*, Table I.), or to two polycladous females and two typical males (classes *f–j*), each tetrad in either case being composed of two and only two types of spores. However, classes *d*, *h*, and *j*, in each of which a clone was secured from only one spore of a tetrad, and classes *c* and *g*, in each of which one clone of each sex appeared, are not conclusive upon this point. Omitting these classes, it appears that there were 7 tetrads (classes *a*, *b*, and *e*) composed of two typical and two polycladous male spores (or, more strictly, of spores bearing the physical bases for these respective characters); and 3 tetrads (classes *f* and *i*) composed of two polycladous female and two typical male spores. These numbers, though small, suggest that there is probably no linkage between polyclady and sex—that is, that the physical bases determining the appearance or non-appearance of polyclady are borne on a pair of chromosomes other than the X–Y pair. This conclusion had been previously reached on the basis of more extensive studies of plants resulting from the broadcast sowing of spore tetrads.

Fourth, there was one clear case (class *k*, Table I.) in which the character-distribution was such that the four spores of the tetrad represented as many different genotypes, and therefore all the combinations possible when two pairs of characters (in this case, female-

male and polycladous-typical) are concerned. In classes *c*, *d*, *g*, *h*, and *j*, the possibility of a similar distribution of factors is not excluded. But, as already seen, it is reasonably certain in 10 cases that the spores of each tetrad fell into only two classes, as compared with one case in which the spores were certainly of four types. It seems safe, therefore, to conclude that, so far as the physical bases of the characters here studied are concerned, in the majority (and probably the large majority) of cases the reduction divisions proceed in such a manner as to result in the production of two and only two differently constituted types of spore nuclei; but that in some (probably few) cases, these divisions bring about the formation of four qualitatively different spore nuclei. The bearing of this conclusion upon the problem of the method of segregation of the chromosomes in the course of the reduction divisions will be considered after the distribution of a second pair of characters has been described.

Tuftedness and Sex.

Another character distinguishing certain strains is designated as "tufted." Tufted strains, both male and female, are characterized especially by aberrancies of varied nature and degree in involucral form. Some branches of a tufted clone bear extremely atypical involucres; other branches of the same clone display a mixture of typically and atypically formed involucres; while still other branches depart in no noticeable way from a strictly typical appearance. There is strong reason for thinking that different tufted clones differ in the magnitude of their inherent tendency to produce aberrant involucres; although, in view of the variations from typical to extremely aberrant forms within a single clone, it is difficult to determine to what extent differences of this nature between clones are genotypic and how far merely phenotypic.

Matings of tufted females by typical males, as previous work has shown, result in f_1 offspring of four classes: typical females, tufted females, typical males, and tufted males. The reciprocal cross (Table II.) gives the same four classes among the f_1 gametophytes. The tufted offspring of such crosses show marked differences in degree of tuftedness, ranging from clones of the total number of whose branches produced during a period of two or three years only a frac-

tion of one per cent. were tufted, to clones which produced during a similar period over ninety per cent. of tufted branches. Evidently, a clone in which the tendency to the production of the aberrancies characteristic of tufted races is present but, for whatever reason, weak, may develop for some time without producing a branch that would be classed as tufted. Such a clone, especially if examined while still small, may, although genotypically tufted, be classed as typical. There is always the likelihood, therefore, in classifying the offspring of a cross of tufted × typical (or typical × tufted), of an error favoring the typical classes. An error in the opposite direction is, as experience has shown, very unlikely.

In Table II. are given the results of the germination of the spores of 47 tetrads, borne by two sporophytes (14 and 33 tetrads respectively) resulting from crosses between typical females and tufted males.

TABLE II.

Typical Female × Tufted Male.

2 Crosses.

21.14 × 20.384

21.215 × 21.16

	Female	Male.	No. Tetrads.	
(*a*)	2 typical,	2 tufted	2	
(*b*)	2 typical,	1 tufted	3	
(*c*)	2 typical	...	4	
(*d*)	1 typical,	1 tufted	2	
(*e*)	1 typical	....	5	
(*f*)	....	1 tufted	1	17
(*g*)	2 tufted,	2 typical	3	
(*h*)	2 tufted,	1 typical	1	
(*i*)	2 tufted	....	7	
(*j*)	1 tufted,	2 typical	1	
(*k*)	1 tufted,	1 typical	2	14
				31
	Apparent Exceptions.			
(*l*)	2 typical,	2 typical	4	
(*m*)	2 typical,	1 typical	3	
(*n*)	1 typical, 1 tufted,	1 typical	4	
(*o*)	1 typical, 1 tufted	......	5	16
				47

All the results shown in Table II., like those of Table I., agree with the expectation of two female and two male clones from each spore tetrad. In 9 cases (classes *a*, *g*, and *l*), all four spores of the tetrad germinated.

In these families, however, three tetrads (not listed in the table) gave results inconsistent on their face with the expectation as to sex. One tetrad produced one typical female plant, and three males of which one was classed as typical, one as tufted, and the third died before its character could be determined. At the time the sporelings from this tetrad were separated, it was noted that two of those which afterward proved to be male (the typical and the uncertain one) were possibly parts of the same plant. When the plants of a tetrad grow close together, uncertainties of this nature occasionally arise, though the greatest care is exercised in separating them. In a second case, three female clones developed from spores of a tetrad. The fact that all three proved to be tufted, contrary to the general rule that not more than two plants of a tetrad are tufted, suggests that here also one sporeling may have been divided when the plants were separated and transplanted. In the third instance, four sporelings were obtained, one of which died when very small; the other three developed into typical female clones. But in this case, the four sporelings as they first appeared were in two distinct groups of two each, suggesting that two spore tetrads may by accident have been placed in the same pot. Thus none of the cases furnishes a certain exception to the general rule as to the distribution of sex characters—although, as pointed out in my previous paper, it would not be surprising if such an exception were to occur now and then in consequence of non-disjunction or other irregularity in chromosome-distribution.

The results from 31 tetrads (classes *a*–*k*, Table II.) fit a supposition that two spores of each tetrad should carry the tufted, and two the typical (non-tufted), potentiality. That this actually occurred is demonstrated in 5 cases (classes *a* and *g*) and very probable in 16 others (classes *b*, *c*, *h*, *i*, and *j*); classes *d*, *e*, *f*, and *k* furnish no evidence on this point. Among the cases listed as "apparent exceptions," those in classes *l* and *m* seem not to agree with such an expectation. These cases will be discussed in a later paragraph.

Classes *a–k* are also in harmony with a supposition that the two female clones derived from any tetrad will possess one vegetative character of the contrasted pair, the two male clones from the same tetrad possessing the opposite character. In 9 cases (classes *a–c*) it is highly probable (certain in class *a*) that the distribution was two typical females, two tufted males; in 12 cases (classes *g–j*), the distribution seems to have been two tufted females, two typical males. There appears, from the near-equality of these two categories, to be no linkage between sex and tuftedness. This conclusion is supported by the results of previous work on clones derived from spores and spore tetrads sown broadcast.

The "apparently exceptional" cases (classes *l–o*, Table II.) remain to be considered. In connection with these, it is to be recalled that a clone genotypically tufted may develop for some time without producing tufted branches. Thus, as previous experience has abundantly demonstrated, it is possible to err by classing as typical a clone that is genotypically tufted, whereas, at least under conditions reasonably favorable to the development of the plants, the possibility of the opposite error seems to be virtually excluded. It is evident that, were certain plants now listed as typical in classes *l–o* to be transferred to the "tufted" category, all the tetrads now placed in these classes would fit the expectation, based upon classes *a–k*, that of each tetrad the two female clones will fall in one category (typical or tufted) and the two male clones in the opposite category. As most of the clones here listed are still living, it is possible that further observations may show some of the apparently exceptional cases to agree with the rules as to the distribution of characters which seem to hold in classes *a–k*.

It is true that classes *l* and *m* could also be explained on the hypothesis that the differences between tuftedness and non-tuftedness are conditioned by two or more factor differences, and that the factors concerned are, or may be, distributed independently in the course of the reduction divisions. But until further observations have reduced the chances of error in these particular cases, the development of this hypothesis is unnecessary.

On the face of the results, the tetrads in classes *n* and *o* may be interpreted as representing the same type of distribution that was

found in one instance among the offspring of a typical-polycladous mating—the appearance, namely, of four types of spores in a single tetrad. In the present case, a distribution of this sort would produce one typical female, one tufted female, one typical male, and one tufted male. While it is quite possible that some of the tetrads of classes *n* and *o* actually represent this type of distribution, the possibility of error mentioned makes such a conclusion at present unsafe. On the other hand, it is clear that, in the majority of cases in these families, each tetrad was composed of two and only two types of spores.

The Method of Chromosome-Segregation.

A central problem of genetic research concerns the method by which, in the course of the reduction divisions, the substance of the chromosomes is distributed between the four resultant nuclei. That such a segregation occurs as to bring about in the respective daughter nuclei genetically different groupings, certainly of the chromosomes themselves, and possibly of parts of chromosomes, is demonstrated cytologically and experimentally. But many open questions remain regarding the precise manner in which this segregation is effected.

One question upon which agreement is not complete concerns which of the two reduction divisions actually brings about the separation of the temporarily paired chromosomes of different ancestral origins. Virtually all the cytological study of plants and the majority of such work on animals indicate that the qualitative segregation of chromosomes is brought about in the first of the two reduction divisions (the heterotypic division), the second (homœotypic) division then being apparently equational. On the other hand, in some animals, especially in a number of Hemiptera, it appears that the heterotypic division is equational, a qualitative segregation occurring in the homœotypic division. The evidence supplied by the experimental study of inheritance seems to show that, whichever is the "reducing" or segregative division, the separation of the chromosomes of one pair is independent of that of the chromosomes of any other pair—the chromosomal constitution of the daughter nuclei formed by this division thus being mathematically predictable.

The consequences of a segregation in either the heterotypic or the homœotypic division may thus be calculated in terms of the genetic constitution of the four spores (or gametes) formed from any mother cell. If, as in the cases considered in the present paper, the diploid generation possesses, borne on two different pairs of chromosomes, the physical bases for two alternative pairs of characters, the chromosome pairs concerned may be designated as A and A′, B and B′. If segregation of the chromosome pairs A A′ and B B′ occurs in the heterotypic division, in any particular case the daughter nuclei will receive either A B and A′ B′, or A′ B and A B′. The homœotypic division will form from each of these chromosome combinations two similar combination; the four resultant spore (or gamete) nuclei of a tetrad will then contain respectively either the chromosome groups A B, A B, A′ B′, and A′ B′; or A′ B, A′ B, A B′, and A B′. Any tetrad thus will consist of but two genetically different types of spores; but there will be two kinds of tetrads and, all told, four types of spores, the two kinds of tetrads and the four types of spores each occurring in approximately equal numbers. Applying this calculation to the distribution of characters involved in Table I., half of the tetrads would give rise each to two typical females and two polycladous males, and half would produce each two polycladous females and two typical males.

If chromosome-segregation occurs in the homœotypic division, then, using the same notation, each of the daughter nuclei formed by the (equational) heterotypic division would receive each of the four chromosomes A, A′, B, and B′. The homœotypic division would produce, from each of these, two daughter nuclei having respectively AB and A′B′, or A′B and AB′. If the homœotypic division should go on in the same fashion in the two sister nuclei formed by the first division, the tetrad would consist of two types of spores; two spore nuclei would contain AB and two A′B′, or two would contain A′B and two AB′. If, however, the division of the two sister nuclei is effected differently, four spores of as many different types would constitute the tetrad; their nuclei containing respectively AB, A′B′, A′B, and AB′. Assuming that the segregation of chromosomes in the homœotypic division of two sister nuclei occurs independently, then, in a sufficiently large number of tetrads, the following chromo-

some-distribution would appear: one fourth of the tetrads would consist of two kinds of spores whose nuclei contain respectively AB and A′B′; another fourth, of two types of spores with the chromosome groups A′B and AB′; and each of the remaining half of the tetrads would consist of four types of spores characterized respectively by the combinations AB, A′B′, A′B, and AB′. Applying these formulæ to the characters whose actual distribution is illustrated by Table I., the expectation would be that one-fourth of the tetrads would produce each two typical females and two polycladous males; one fourth would produce each two polycladous females and two typical males; and one half would produce each one typical female, one polycladous female, one typical male, and one polycladous male.

The results reported in the present paper obviously do not agree with either of the calculated results just outlined. The three sorts of tetrads, to be sure, occur that would be expected from the second hypothesis; but, instead of tetrads composed of four spores constituting half the total number, tetrads of this sort are evidently exceptional. The rarity of such tetrads might indeed be explained by assuming segregation in the homœotypic division with the additional provision of a tendency for the segregation of the chromosomes to occur in the same way in two sister nuclei. For the latter assumption, however, there is no present observational basis; and the notion of segregation occurring only in the homœotypic division is negatived by all that is known of the reduction divisions in plants. Probably the present observations can be best harmonized with other known facts by supposing that in general (in *Sphærocarpos*) the heterotypic division effects the segregation of chromosomes; but that the additional possibility exists of a segregation at some times or to some extent of chromosomes or parts of chromosomes in the course of the homœotypic division.

The only previous attempts, so far as I know, to determine the time of segregation from the genetic constitution of the spores formed by the reduction division are by Kniep (1922) and Wettstein (1923). The former author has isolated the groups of four spores borne by single basidia of *Aleurodiscus polygonius*. Four genetically different types of spores, as judged from the behavior in conjunction of mycelia derived from them, were produced in the same fruit

body, but in no case were more than two types of spores (two of each type) borne by one basidium. Kniep concludes that the spores differ with respect to two pairs of alternative factors; and that the production of but two types by any one basidium demonstrates that chromosome-segregation occurs in the heterotypic division. Wettstein, by treatment of the developing capsules of hybrid sporophytes of *Funaria hygrometrica*, induced the spores of a tetrad to remain together, instead of separating, as they ordinarily do, at or before maturity. He found that each of 35 tetrads was composed of but two genetically different types of spores, and came to the same conclusion as did Kniep regarding the time of chromosome-segregation. *Aleurodiscus* and *Funaria*, therefore, do not, so far as present knowledge goes, illustrate the possibility demonstrated in *Sphærocarpos* of a segregation in both reduction divisions.

Observations and hypotheses advanced by previous writers suggest at least two ways in which segregation may conceivably be effected in both the heterotypic and the homœotypic division. One possibility is indicated by the cases in insects reported by Wilson (1912), Miss Carothers (1913), McClung (1914), and Wenrich (1916), in which the segregation of certain chromosome pairs seems to occur in the heterotypic division and that of certain other pairs to occur in the homœotypic division. Even should such a difference in the behavior of individual chromosomes be found general, however, it would not account for the proportions in which the different types of character-distribution seem to occur in *Sphærocarpos*, so long as each particular pair of chromosomes is always separated in one or the other division. Wenrich's conclusion that a particular pair of chromosomes may be separated sometimes in one division, sometimes in the other, might, on the other hand, account for any conceivable variation in the proportions of the types of different nuclei formed by the reduction divisions.

Another possible explanation of the present results is to be found in Janssens' (1909, 1919 *a*, *b*) theory of chiasmatypy. It is true that the major use of this theory by Morgan and others has been in connection with genetic evidences of a crossing over which may be conceived as occurring at a time when the paired chromosomes are

not longitudinally split. But Janssens' own development of the theory emphasized the possibility of a crossing over between the respective halves of the split chromosomes of a pair, in such a way as to make possible a differential segregation of parts of chromosomes in both reduction divisions. The conception that chiasmatypy may bring about results of this nature is supported by the genetic evidence in such cases as one reported by Bridges (1916), in which the genetic results are explainable by a crossing over in the "four-strand" stage.

It would be useless at present to attempt to determine whether the distribution of characters among the tetrad sibs of *Sphærocarpos* best fits one or the other of these conceptions, or possibly some entirely different explanation, of the phenomena of the reduction divisions. That in some way qualitative segregation can be brought about in both divisions seems a reasonable inference from the facts here presented. There is reason to hope that further observations may materially extend the scope of the method of analysis here outlined.

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YELLOW FEVER AND FISHES IN COLOMBIA.[1]

By C. H. EIGENMANN.

Read April 25th, 1924.

Dr. H. Hanson, of the Rockefeller Foundation, who two years ago fought yellow fever in Peru, has transferred his operations against yellow fever to Colombia. He has kindly sent me a series of the fishes he has found useful or is trying out. Comparing the species received from Colombia with those used by him in Peru and those used by Drs. Pareja and Connor in Ecuador, some general conclusions may be pointed out.

The two essentials to make a fish useful for yellow fever work are: (1) The inclination to eat mosquito larvæ. (2) The ability to exist in the containers in which yellow fever mosquitoes breed.

No doubt scores of species fulfill these two requirements in different parts of the fever-infested or potential fever region. Which ones are best in a given area will have to be worked out experimentally. This much seems clear. Other things equal, small species of fishes will be more suitable than large species, though the young of some large species may answer the purpose very well.

Other things being the same, species living in ponds, or stagnant water, are more likely to be serviceable than those from swift water.

Some species pick up their food by sight. Other things equal, they may be quite effective in removing moving mosquito larvæ during daylight.

Other species detect their food by its motion. Such species are usually provided with elaborate tactile organs either in the skin itself, like the blind-fishes, or in barbels like various catfishes. These may also be useful, frequently operating at night.

It will probably be found that many of the fishes imported by aquarists will be suitable for mosquito work in the tropics.

[1] Contribution from the Zoölogical Laboratory, Indiana University, No. 202.

The most effective mosquito larvæ eradicator in Peru is "Life," *Pygidium piura*. In Colombia, near Bucaramanga, a smaller species of the same genus is being used, *Pygidium striatum*. Every mountain brook in Colombia contains one or more species of Pygidium, if they are really all distinct. They sometimes are found under rocks in swiftly flowing water. I found one species in a little brook near Honda diving into the sand and small gravel at the bottom. When disturbed or routed out of one spot, they quickly darted into another. In the uplands they have a tendency to live in quieter water. They have barbels and the Peruvian *P. piura* operates at night.

The "Volador" (*Piabucina panamensis*) used in Colombia is a close relative of one of the most effective mosquito eradicators used in Lima, Peru, under the name of *Liza de agua dulce*, in Ecuador as "Huaijas" and at Chiclayo, Peru, as "Chalquoque."[2] The name "Volador" suggests the reputation of "Chalquoque" which has a tendency to jump out of its containers. This habit no doubt contributed to its distribution. It has recently been taken in a line entirely across the Andes from Pacasmayo to Cajamarca and down to the Marañon, and is the only species of fish with such an extravagant distribution.

The "Mero" is being used at Barranquilla. We became acquainted with its next door relative in the yellow fever work at Guayaquil under the name "Chalacos." The Pacific slope form is *Dormitator latifrons*, that in Barranquilla and along Atlantic slope fishes generally is *Dormitator maculatus*. The Pacific slope form proved very effective and there is every reason to believe that the Atlantic slope form will prove equally so,[3] if it is found in sufficient abundance.

The three species of fishes used most effectively in Peru and Ecuador have counterparts, close relatives systematically, which are doing the work in Colombia.

Another species being used in Colombia is a "Mojarra." The Mojarras are sunfish-like fishes. There are several genera and species. Dr. Hanson experimented with *Æquidens rivulatus* in Peru.

[2] See *Am. Nat.*, LVII., pp. 440 and 447, for figures and distribution map.
[3] See *Proc. Am. Philos. Soc.*, LXI., p. 204, 1922.

The species being tried out in Colombia is *Geophagus steindachneri.* There are two, possibly three, species of Geophagus in Colombia, one in the Atrato-San Juan, the other in the Magdalena basin up to Girardot. One species is found in Panama and other species are found east of the Andes. They are very active, nervous fishes, dodging in among and hugging the sides of rocks. From their general habits, I should judge them effective sight hunters of larvæ, but I fear they will prove too nervous for the work in small containers, frequently disturbed. The young may survive in the hampered environment of the containers. The males carry the eggs and very young in their mouths.

Two species of " Catfishes " with long whiskers are being used in Colombia, *Pimelodella chagrensis* and *Rhamdia sebæ.* These are fishes of the lowlands and quiet pools, living in general in a very different environment from Pygidium. They have flexible barbels, more than half as long as the entire fish, with which they can (theoretically at least), explore any small container with the minimum of muscular effort. Another species of the genus Pimelodella (*yuncensis*) was tried by Dr. Hanson in Peru but was abandoned.

Dr. Hanson has mentioned two other species, the " Boca chica " and a brackish water species near Cartagena. The " Boca chica " of the Magdalena, *Prochilodus magdalenæ,* ascends the river in countless millions during winter. They are dried and used by laborers as food. The adult is certainly not suitable for yellow fever work. The young may serve. Another species of the same genus was tried in Ecuador, *Prochilodus humeralis,* but abandoned.

The brackish water species is possibly *Mollienisia sphenops.* Another species of the same genus, *Mollienisia caucana* is abundant in the swamps of the lower Magdalena basin. It is a minute fish that for *a priori* reasons should make an ideal species to do the work.

Species of the Pœciliidæ, to which Mollienisia belongs, and a family which contributes mosquito eradicators elsewhere, are widely distributed in Colombia. Along the coasts of the Guianas they are especially abundant both in species and numbers.

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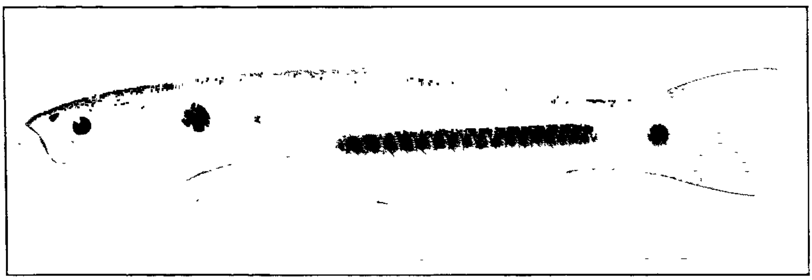

Piabucina panamensis Gill. The "Volador" of yellow fever work in Colombia.

THE BEHAVIOR OF *ŒNOTHERA NEO-LAMARCKIANA* IN SELFED LINE THROUGH SEVEN GENERATIONS.[1]

BY BRADLEY MOORE DAVIS,

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I reported in an earlier paper (Davis 1916*b*) the segregation of a hybrid in the F_3 generation from the cross *Œnothera franciscana* × *Œnothera biennis* with so many points of resemblance to *Œnothera Lamarckiana* that it seemed to me to be scarcely distinguishable from *Lamarckiana* as this collective species would be described in systematic botany. I have named this hybrid *Œnothera neo-Lamarckiana* and have carried it along in selfed line through seven generations. This paper will give (1) The behavior of *neo-Lamarckiana* over this period of time, (2) The results of crosses to *biennis* designed to test its purity, and (3) The behavior, when selfed, of the more prominent derivatives from *neo-Lamarckiana*.

We shall note that the behavior of *neo-Lamarckiana* in throwing through successive generations certain characteristic types is similar to that of *Lamarckiana* when it produces with each generation its particular set of "mutants." Morgan (1923, pp. 309, 310) sees little point in this parallelism of behavior because certain wild species of *Œnothera* studied by de Vries, Stomps and Bartlett show behavior of the same kind as that of *Lamarckiana*. Morgan, however, apparently overlooks the evidence which indicates that these wild species like *Lamarckiana* are heterozygous and may not be taken as representatives of pure species (Davis 1922). They together with *Lamarckiana* are illustrations of impure species, of which the genus *Œnothera* seems almost wholly to be composed, impure species which chiefly breed true because for the most part only such gametes are developed and produce vigorous zygotes as reproduce thereby the heterozygous constitution of the parent stock.

The parent species of *neo-Lamarckiana* were *Œnothera fran-*

[1] "Genetical Studies on *Œnothera*," XII. Papers from the Department of Botany, University of Michigan, No. 204.

ciscana Bartlett from California (Bartlett 1914) and *Œnothera biennis* Linnæus from Holland (Bartlett 1913), and these species have been fully described and figured as to their contrasting characters (Davis 1916*a*, pp. 206, 207). Their points of resemblance to *Œnothera Lamarckiana* in various particulars have also been shown (Davis 1916*a*, pp. 210–212). The F_1 generations of reciprocal crosses were compared in an earlier paper (Davis 1914, pp. 190, 191) and will not be discussed further. A set of inter and back crosses involving the F_1 hybrids and the parent species has also been described (Davis 1917, pp. 170–178).

It is a matter of interest that my selfed line of *franciscana*, which has now been carried through ten generations, when pollinated by *biennis* still continues to give the same type of F_1 generation as was obtained in the first cross made in 1912 out of which came the vigorous plant *neo-Lamarckiana* (Davis 1914, pp. 189, 190). This cross in recent years has been repeated three times and always large proportions of the rosettes have been pale green and weak, with mortality so high that it is difficult to bring even the strongest plants to maturity.

This account will begin with a plant 14.53*c* in an F_2 generation. The culture 14.53 was one of five parallel F_2 generations from five sister plants of the F_1 culture 13.35, *franciscana* B × *biennis* (Davis 1914, pp. 190, 191). The five sister F_1 plants (13.35*ac*, 13.35*b*, 13.35*c*, 13.35*g* and 13.35*r*) differed from one another as has been noted (Davis 1916*a*, p. 229) and gave F_2 generations with various peculiarities (Davis 1916*a*, pp. 228–242). The plant 14.53*c* was selected as most likely to give in succeeding generations some *Lamarckiana*-like offspring.

The F_2 Generation.

The F_2 generation, culture 14.53 from the F_1 plant 13.35*c*, was from a sowing in earth of 454 seeds from 5 capsules; 387 seedlings appeared from which 376 rosettes developed, and 363 plants reached maturity. The culture as rosettes separated into two groups, 108 plants with obtuse pointed leaves and 268 plants with acute pointed leaves. From the rosettes with obtuse pointed leaves came broad-leaved plants with small flowers of the *biennis type* in having stigmas below the anther tips; a single *hero* plant appeared from this group

and also an obtused-leaved dwarf. The rosettes with acute pointed leaves developed plants agreeing in having foliage of narrow pointed leaves, but exhibiting a range of flower structure from that close to *biennis* to that of the *franciscana type;* 10 dwarfs characterized by sickle-shaped rosette leaves were in this group.

There were several plants in the group with acute pointed leaves which presented in different respects and in varying degrees characters of *Œnothera Lamarckiana.* The best of these *Lamarckiana*-like plants, 14.53*c*, was selected to carry forward a selfed line and its characters contrasted with those of races of *Lamarckiana* with which I am familiar are given below.

Lamarckiana.	*Plant 14.53c.*
Seedlings. Cotyledons broad at base and almost sessile.	*Seedlings.* Cotyledons similar to those of *Lamarckiana.*
Mature rosettes. 4–5 dm. broad. Leaves broadly elliptical or spatulate with sinuate margins, irregularly toothed below, strongly crinkled. Midveins white, in some races reddish.	*Mature rosettes.* 4 dm. broad. Leaves as in *Lamarckiana.* Midveins white.
Mature plant. Central stem about 15 dm. high. Long side shoots from the rosette, and side branches about midway up the central stem. Upper foliage of ovate-lanceolate leaves, short petioled, crinkled. Midveins white, in some races reddish. Stems red papillate.	*Mature plant.* Central stem 9 dm. high. 6 side shoots from the rosette, 5–8 dm. long, side branches from central stem fewer and shorter. Upper foliage of leaves not so strongly ovate at base, otherwise as in *Lamarckiana.* Midveins white. Stems red papillate.
Inflorescence. Compact and flat topped. Bracts sessile, broad at base, early in the season equaling the length of the buds, later much shorter.	*Inflorescence.* Bracts early in season 2/3 the length of the buds. Otherwise as in *Lamarckiana.*
Buds. In large-flowered races about 8 cm. long. Cone stout, 4-angled. Sepals green, in some races streaked with red. Sepal tips attenuate, 6–8 mm. long. Pubescence on sepals pilose and puberulent consisting of long hairs arising from papillæ among numerous short sessile hairs. Ovaries with red papillæ.	*Buds.* 7.5 cm. long, cone somewhat less stout and less strongly 4-angled. Sepals green. Sepal tips attenuate, 5–6 mm. long. Pubescence as in *Lamarckiana* but more dense. Ovaries with red papillæ.

Petals. 4–4.5 cm. long. in some races as small as 3 cm.
Stigma lobes. Usually 4, sometimes 5 or 6. Style extending 5–7 mm. above the tips of the anthers.
Capsules. In some races stout and about 2 cm. long, in others more attenuate and about 3 cm. long.

Petals. 3.3 cm. long.
Stigma lobes. 4, rarely more. Style extending 2–3 mm. above the tips of the anthers.
Capsules. About 2 cm. long.

From this comparison of hybrid 14.53*c* with *Lamarckiana* it will be noted that the differences between the two plants were plus and minus degrees in the expression of similar characters. The most important points of resemblance were those of rosette, foliage, compact inflorescence, flower size, stigmas projecting beyond the tips of the anthers, capsule form and size (Fig. 1), pubescence in

FIG. 1. Capsules of plant 14.53*c*, *Lamarckiana*-like hybrid in the F_2 *franciscana* × *biennis*, compared with the capsules of *Lamarckiana*.

general, and red papillæ over the stems and ovaries. The important differences of the hybrid were the more sprawling habit of growth of the mature plant, its bud cones less stout, and its shorter sepal tips with their heavier pubescence. There were few plants in this F_2 generation *Lamarckiana*-like; in fact only 3 were considered worthy of attention.

The evidence of segregation was clear throughout this F_2 generation as shown in the great range of flower size and structure, with the extremes approaching the *biennis* and *franciscana types*, and in the varied foliage. All of the plants had the red-papillate stems of *franciscana;* no green stems as in *biennis* were represented in this culture which, however, being earth sown, probably did not exhibit the full extent of segregation possible from the F_1. A culture from the sister F_1 plant, 13.35*ac*, from seeds germinated in Petri dishes (culture 15.51) gave a small group of green-stemmed segregates (Davis 1916*a*, pp. 229–230). We shall see that the class of dwarfs, represented by 10 plants, appears constantly in succeeding generations, and from time to time also the *hero type.* Later there became more sharply differentiated the large class of plants *franciscana*-like in habit, foliage and bud structure.

THE F_3 GENERATION.

The F_3 generation, culture 15.53 from the F_2 plant 14.53*c*, was grown from 625 earth sown seeds, the contents of 4 capsules. The seed pans, kept for $7\frac{1}{2}$ weeks, yielded 291 seedlings, a germination of only 46.6 per cent. That this was a low percentage and a poor germination was proved later by germinating in a Petri dish a set

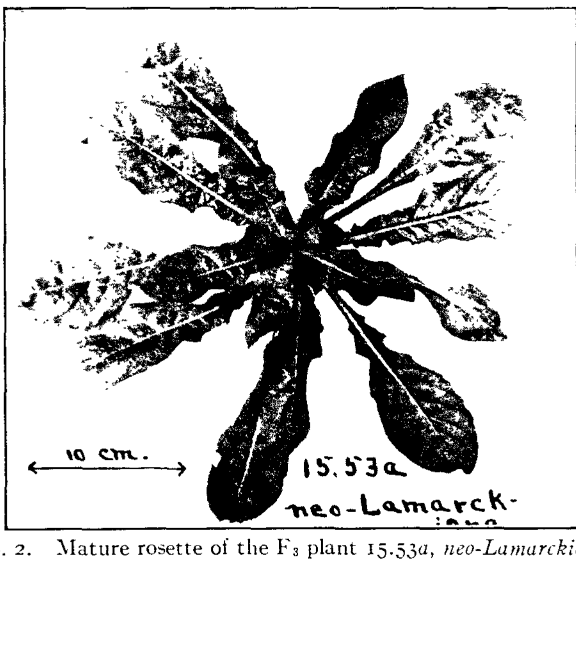

FIG. 2. Mature rosette of the F_3 plant 15.53*a*, *neo-Lamarckiana*.

of 644 seeds which gave 574 seedlings, a germination of 89.1 per cent. The rosettes, as in the F_2 generation, were readily separated into two groups, 91 plants with obtuse-pointed leaves and 197 plants with acute-pointed leaves; there were 3 dwarfs. In the group with acute pointed leaves 7 plants developed *Lamarckiana*-like rosettes and became at maturity *Lamarckiana*-like in other respects; From these a plant, 15.53*a*, was selected to be parent of the next generation and its characters are here considered and illustrated in Figs. 2, 3 and 4. With this generation I first began to use for such types the name *neo-Lamarckiana*.

Plant 15.53a, neo-Lamarckiana.

Mature rosette. As shown in Fig. 2 the mature rosette had all of the characteristics of *Lamarckiana*.

Mature plant. Central stem well developed, at maturity 13 dm. high. The habit of the plant (Fig. 3) and its foliage were quite the same as those of *Lamarckiana*. Stems red papillate.

FIG. 3. Mature F_3 plant 15.53*a*, *neo-Lamarckiana*.

Inflorescence. Compact and flat topped (Fig. 4) as in *Lamarckiana.* Bracts similar to those of *Lamarckiana.*

FIG. 4. Inflorescence of F_3 plant 15.53*a*, *neo-Lamarckiana.*

Buds. 8 cm. long. Cone (Fig. 4) more slender than in *Lamarckiana*, 4-angled. Sepals with faint reddish streaks. Sepal tips with heavier pubescence than in *Lamarckiana* but of similar character. Ovaries red papillate.

Petals. 3.7 cm. long.

Stigma lobes. Style extending 3 mm. above the tips of the anthers.

Capsules. About 2.4 cm. long.

As compared with plant 14.53*c* of the F_2 generation plant 15.53*a* was closer to *Lamarckiana* in presenting a much better habit of growth with a tall central stem and properly arranged side shoots (Fig. 3), in better foliage now beyond criticism and in larger petals. The bud cones of the hybrid (Fig. 4) were, however, less stout and the shorter sepal tips had a heavier pubescence.

Segregation was expressed in the F_3 generation in much the same manner as in the F_2, showing itself most conspicuously in the size of petals, length of style, and form of leaves. The rosettes with obtuse pointed leaves generally developed plants with foliage and flowers more *biennis*-like although there was a wide range of variation and 9 plants were clearly more *franciscana*-like as to these characters; in all of the plants of this group the bud tips were thick and showed the heavier pubescence peculiar to *franciscana* and capsules were *franciscana*-like as to their length, about 3 cm. The rosettes with acute pointed leaves, except for the 7 plants of *neo-Lamarckiana*, gave *franciscana*-like plants as to their bud tips, foliage and capsules; variation here expressed itself in flower size and style length, and 52 plants in this group were more *biennis*-like in these particulars. Thus the promise held out by a rosette as indicating a plant more *biennis*-like or more *franciscana*-like was not realized in all characters of the adult plant. The mass of the culture agreed in having the red-papillate stems, bud tips, and capsule length peculiar to *franciscana*. Green-stemmed segregates were lacking but the germination of seeds was very far from complete and some types of segregation may well have been absent. The 3 dwarfs, similar to those present in the F_2 generation, constituted a numerically small group probably because of the unsatisfactory germination of the earth-sown seeds. No *hero types* appeared. The 7 *Lamarckiana*-like plants stood in a group apart and were recognized and separated as rosettes from the mass of the culture.

THE F_4 GENERATION, AND THE FIRST GENERATION FROM *Neo-Lamarckiana*.

This was the first generation of *neo-Lamarckiana* grown from seeds forced to complete germination, and is of particular interest for the range of segregation displayed and for the numerical proportions of some of the classes. The culture, 16.41, came from selfed seeds of the F_3 plant 15.53*a*, the first representative of *Œnothera neo-Lamarckiana* (Figs. 2–4).

The contents of 3 capsules, 764 seeds, after soaking for 40 hours, were subjected, while still in water, to pressure of about 100 pounds in an iron case for 36 hours. The seeds were then spread on wet

pads of filter paper in Petri dishes and left at room temperature. Germination began at once and was mostly completed after 10 days. The seedlings with their empty seed coats were removed from the Petri dishes as germination proceeded and the seedlings set out in pans. The remaining ungerminated seeds, after 30 days in the Petri dishes, were tested for contents by pinching with strong forceps and found to be empty. From this culture of 764 seeds there came 668 seedlings, a germination of 87.4 per cent. This is interesting in contrast to the record of the earth-sown seeds of the F_3 generation which gave a germination of only 46.6 per cent.

A very large number of the seedlings after sprouting were unable to throw off their seed coats and it was necessary to remove the coats by teasing and pressure. This operation was responsible for an early mortality of many seedlings which together with the death of 6 seedlings with etiolated leaves and a number of feeble dwarf rosettes with narrow leaves reduced the culture to a total of 549 plants when set out in the garden.

As the rosettes of this culture developed it was apparent that a very much larger number gave some promise of becoming *Lamarckiana*-like than in the cultures of the F_2 and F_3 generations, and a group of 194 rosettes were thus segregated before the plants were set out. Of this group 36 rosettes developed into large plants of the *neo-Lamarckiana type* with central stems 10–13 dm. high, 65 plants less vigorous presented stems ranging from 3–9 dm. high, or failing to develop central stems put out *Lamarckiana*-like side shoots from the rosettes, 48 plants remained as *Lamarckiana*-like rosettes, 29 rosettes became dwarfs generally with narrow sickle-shaped leaves and of these none developed flowering shoots; 16 rosettes died. Thus of the 194 plants in the entire group which started out with indications of *Lamarckiana*-like characters only 36 came out as thoroughly good representatives of *neo-Lamarckiana*. The *neo-Lamarckiana* plant selected to be parent of the F_5 generation was 16.41, XIX–4. Its characters were those described for plant 15.53*a* of the F_3 generation and illustrated in Figs. 2, 3 and 4.

An assemblage of 355 rosettes with narrow leaves was left after the selection of the 194 rosettes with *Lamarckiana*-like characters. This group produced the following varied assortment of types; 32 weak dwarf rosettes with narrow sickle-shaped leaves only 3 of

which lived to send up short shoots, 17 narrow-leaved dwarf rosettes which failed to reach maturity and probably were in the same group as the first although they had symmetrical leaves, 306 *franciscana*-like rosettes of which 289 lived to develop plants which will be designated the *franciscana type*, distinguished as follows:

The franciscana type.

Mature rosettes. Narrow leaves similar to those of *franciscana*.

Mature plants. Habit as in *franciscana* and with the same type of narrow leaves. Frequently plants are larger leaved and more robust. Stems red papillate.

Inflorescence. As in *franciscana*.

Buds. About 7 cm. long but variable. Sepals streaked with red and heavily pilose and puberulent as in *franciscana*. Sepal tips thick, blunt pointed as in *franciscana* and with similar heavy pubescence.

Flowers. Generally large as in *franciscana*, but variable in size.

Stigma. Generally about 3 mm. above the anther tips, but sometimes at their level or below.

Capsules. About 2.8 cm. long as in *franciscana*. Large yield of seed.

Pollen. Almost wholly perfect.

Many plants of this type were so close to *franciscana* that they would fall well within a description of the species. Others depart from the type as noted above in their more robust habit and somewhat larger leaves, variable flower size and position of the stigma. Characteristic and important points of resemblance are the similar bud tips and similar bud pubescence. This *franciscana type* was represented also in the F_2 and F_3 generations by large numbers of plants and in later generations it stands out as a well-differentiated segregate.

The dwarfs with narrow, frequently sickle-shaped leaves were always numerous in generations grown from seeds forced to complete germination. They were represented in later generations by many mature plants and will be referred to as the *franciscana dwarf type* with the following characters:

The franciscana dwarf type.

Mature rosettes. Dwarf, 10–15 cm. broad. Leaves generally narrow and frequently crinkled and sickle shaped (Fig. 5).

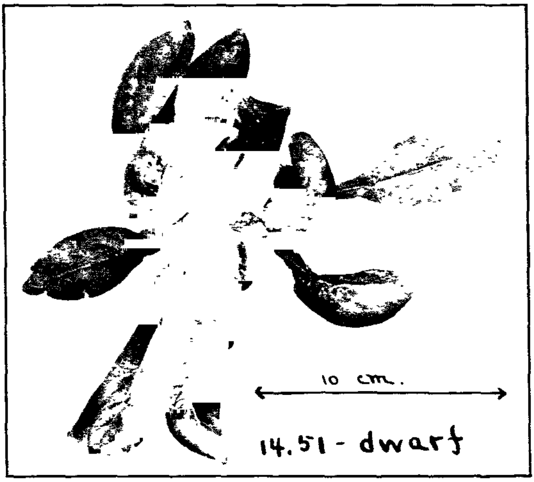

FIG. 5. Mature rosette, 14.51*d*, representative of the *franciscana dwarf type.* Leaves narrow and frequently sickle shaped.

Mature plants. 2–3 dm. high, few sprawling branches, leaves narrow and acute pointed.

Inflorescence. Few flowers in a loose spike.

Buds. Short blunt sepal tips with *franciscana* pubescence.

Flowers. Small, petals 8–15 mm. long.

Stigma. Generally at level of or below the anther tips.

Capsules. Small, with low yield of seed.

Pollen. Almost wholly perfect.

Seedlings and dwarf rosettes with narrow etiolated leaves have always appeared in cultures of *neo-Lamarckiana* from seeds forced to complete germination. Because the type resembles *albida* from *Lamarckiana* it has been called *albida*-like.

Albida-like.

Seedlings with etiolated cotyledons which develop dwarf rosettes with narrow etiolated leaves. They are weak plants which generally die early as seedlings or as young rosettes. I have never been able

to bring these plants to maturity even with particular care and under the protection of the hot house.

A matter of interest in this history of the line of *neo-Lamarckiana* is the disappearance in this generation of the class of plants with obtuse pointed leaves, both rosette and foliage, which was conspicuous in the F_2 and F_3 generations. Evidently the genetical make-up of plant 15.53*a* from the F_3 generation was different from that of the plants 14.53*c* and 13.35*c* which preceded it in the line, and a new genotype was selected at this time. This was the genotype of *neo-Lamarckiana* which has continued with apparently few if any modifications through the later generations.

The composition of the F_4 generation presented the following distribution of plants:

36 plants of *neo-Lamarckiana*, 5.4 per cent.
65 plants less vigorous, *Lamarckiana*-like in foliage but with bud tips more like *franciscana*.
48 plants remained as *Lamarckiana*-like rosettes.
6 etiolated seedlings and rosettes, *albida*-like. These died early.
289 plants of the *franciscana type*, 43.3 per cent.
78 plants of the *franciscana dwarf type*.
146 plants died before their characters could be determined, 113 as seedlings or young rosettes and 33 after the culture was set in the ground.

668 Total of germinations, 87.4 per cent.

In summary of the F_4 generation it should be pointed out that segregation was displayed on a much larger scale than is indicated in this short account. It is clear that the large group of 194 rosettes, selected when half grown as *Lamarckiana*-like, was a heterogeneous assemblage, since only 36 at maturity were *neo-Lamarckiana;* of the others 29 became dwarfs, a large number approached *neo-Lamarckiana* but failed to qualify as to flower structure, 48 remained as rosettes, and 16 died. It is highly probable that a number of different genotypes were represented as segregates and might have been isolated if time and garden space could have been given to their further study. With respect to the large group of

355 rosettes with narrow leaves much the same situation was presented. There was a group of 49 dwarfs and in the large assemblage of 306 rosettes of the *franciscana type* sufficient variation appeared as the plants matured to indicate that with respect to flower size and position of the stigma the group was far from homogeneous. It was not my purpose in this study to trace the ramifications of segregation but rather to study the results of selection to the type of *neo-Lamarckiana*.

THE F_5 GENERATION, AND THE SECOND GENERATION FROM *Neo-Lamarckiana*.

The F_5 generation was studied with particular care as to the character of its segregates, and, like the F_4 generation, was grown from seeds forced to complete germination. The culture, 17.41, was from selfed seeds of the F_4 plant 16.41, XIX–4, the second plant in the line of individuals bearing the name *neo-Lamarckiana*.

The contents of 5 capsules, 1,089 seeds, were soaked 24 hours and then subjected while in water to pressure of about 100 pounds for 36 hours. The seeds were germinated in Petri dishes at first at room temperature and later placed in the greater warmth of the green house. The culture gave 899 seedlings, a germination of 82.5 per cent.

It was necessary to remove the seed coats from 109 germinating seeds the cotyledons of which were unable to free themselves, and of this group 65 seedlings shortly died in the seed pans. Thus only 44 seedlings survived the operation and these produced a mixed group of rosettes showing that they did not constitute a genetical class. Other mortality in the culture eliminated early 19 more seedlings.

The culture in the rosette stage contained 815 plants which were grouped as follows: 277 *Lamarckiana*-like rosettes, 52 dwarf rosettes frequently with sickle-shaped leaves, 452 *franciscana*-like rosettes, 34 etiolated dwarf rosettes, *albida*-like, which died early. It will be observed that the class of rosettes with obtuse pointed leaves which dropped out in the fourth generation did not reappear. As in other generations this classification of rosettes although of interest did not give the final composition of the culture since many modifications appeared as the plants matured.

From the group of 277 *Lamarckiana*-like rosettes 91 plants of *neo-Lamarckiana* matured, 102 plants bore *Lamarckiana*-like foliage but were more *franciscana*-like as to their bud tips, 22 plants of the *hero type* appeared, 13 plants developed of the *franciscana dwarf type*, 18 plants remained as *Lamarckiana*-like rosettes, 31 plants died.

The group of 52 dwarf rosettes, frequently with sickle-shaped leaves, produced 31 plants of the *franciscana dwarf type*, and 6 plants *nanella*-like in foliage and bud characters; 15 plants died.

In the large assemblage of 452 *franciscana*-like rosettes 9 plants of the *franciscana dwarf type* appeared; 410 typical rosettes were discarded for lack of garden space and the remaining 33 rosettes became plants of the *franciscana type*. The discarded rosettes were undoubtedly chiefly if not wholly of the *franciscana type* which was, therefore, represented in the culture by a very large group.

Thus there appeared in this highly varied assemblage of the F_5 generation the same clearly defined segregates as in the F_4, and in addition two new forms, *e.g.*, a *nanella*-like dwarf and the *hero type* which, however, was represented in the F_2 by a single plant. The former segregates which reappeared were the *albida*-like dwarfs, the *franciscana type*, the *franciscana dwarf type*, and the probably mixed assemblage of forms with *Lamarckiana*-like foliage but with buds more *franciscana*-like, and various in flower size. The *nanella*-like plant and the *hero type* are distinguished as follows:

Nanella-like.

Mature rosettes. Medium-sized with broad leaves closely set and frequently asymmetrical.

Mature plants. About 3 dm. high, compact with short internodes, foliage of broad leaves.

Buds. Sepal tips pointed.

Flowers. Various in size.

Capsules. Medium-sized and good yield of seed.

Pollen. About 90 per cent perfect.

The hero type.

Mature rosettes. Very large, the leaves broad and thick (Fig. 6).

Mature plants. With long spreading side shoots (Fig. 7), foliage of large thick leaves.

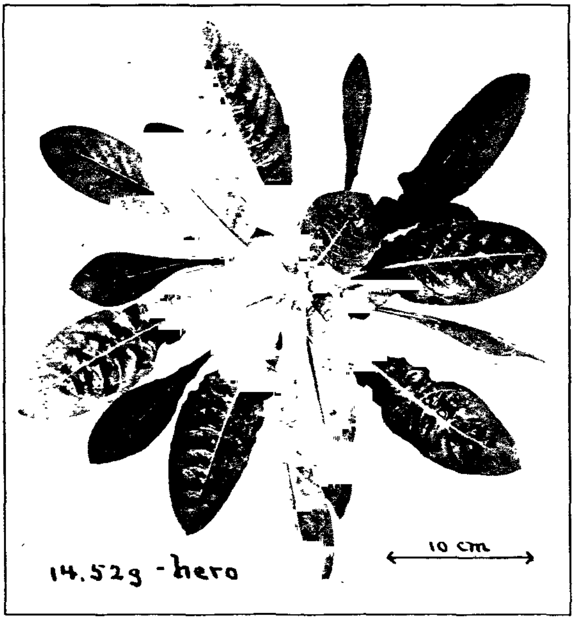

FIG. 6. Mature rosette, 14.52g, representative of the *hero type* with the triploid number of chromosomes (21). Remarkable for the breadth and thickness of the leaves.

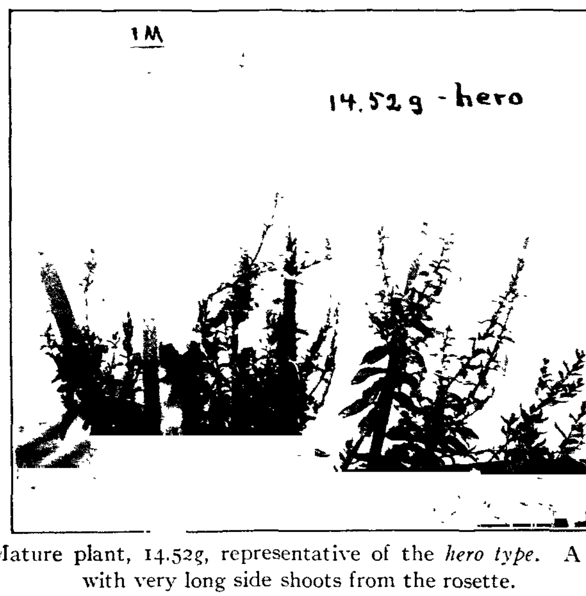

FIG. 7. Mature plant, 14.52g, representative of the *hero type*. A large plant with very long side shoots from the rosette.

Inflorescence. As in *franciscana*, elongate and not flat topped (Fig. 8).

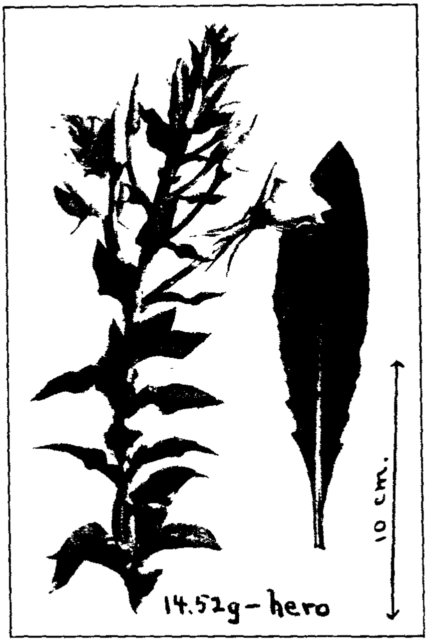

FIG. 8. Inflorescence of the *hero type*, 14.52g. Long buds with stout cones.

Buds. About 8 cm. long, stout (Fig. 8). Sepal tips thick, blunt pointed and with the heavy pubescence of *franciscana*.

Flowers. Petals 3.8 cm.

Stigma. Lobes 7–8 mm. long, 5–6 mm. above the anther tips.

Capsules. About 1.8 cm. long, yielding at best a very small harvest of seed when selfed, and these seeds largely sterile.

Pollen. Small in quantity and the grains chiefly shriveled.

The *hero type* from *neo-Lamarckiana* was a plant of such interest that several tests were made of its fertility and behavior in selfed lines (Table III.). Chromosome counts in the developing ovules of two plants gave numbers ranging from 17–20 which, together with the great variation displayed in selfed lines and the very low fertility, clearly indicates that the *hero type* is generally a triploid plant carrying 21 chromosomes. One plant (20.43–3) proved to have 28 chromosomes and, therefore, in this respect corresponded

to *gigas* from *Lamarckiana*. This plant gave culture 21.43 of Table III. which unfortunately suffered such mortality as to produce no mature plants and consequently the line ended.

The composition of the F_5 generation presented the following distribution of plants:

91 plants of *neo-Lamarckiana*, 10.1 per cent.
102 plants *Lamarckiana*-like in foliage but with bud tips more like *franciscana*.
18 plants remained as *Lamarckiana*-like rosettes.
6 dwarfs, *nanella*-like in foliage and bud characters.
34 etiolated seedlings and rosettes, *albida*-like. These died early.
22 plants of the *hero type*.
443 plants of the *franciscana type* including 410 typical rosettes discarded for lack of garden space, 49.3 per cent.
53 plants of the *franciscana dwarf type*.
130 plants died before their characters could be determined, 84 as young seedlings and 46 older plants either as potted rosettes or in the field.

899 Total of germinations, 82.5 per cent.

The plant of *neo-Lamarckiana*, 17.41, XVIII–2, selected to carry forward the line was wholly true to its type.

The F_6 Generation, and the Third Generation from *Neo-Lamarckiana*.

The F_6 generation, culture 19.41, was from selfed seeds of the F_5 plant 17.41, XVIII–2, the third plant in the line of individuals bearing the name *neo-Lamarckiana*. Like the F_5 generation it was studied with particular care as to the nature of the segregates and the culture was grown from seeds forced to complete germination.

From 3 capsules 701 seeds were obtained which after soaking for 24 hours were placed, during another 24 hours while in water, twice under pressure of 75 pounds and twice under exhaust. This alternation of pressure and exhaust was a modification of former treatment and designed to bring about more quickly the entrance of water into the seed. Germination was immediate and almost

ended after a week; it was fully completed after 3 weeks. The seedlings numbered 611, a germination of 87.2 per cent.

A group of 14 etiolated, *albida*-like seedlings and young rosettes died early which together with other mortality partly due to the operation of removing persistent seed coats reduced the number of plants to 586 at the time when the culture was set in the ground. These 586 plants were at this time grouped as follows: 156 *Lamarckiana*-like rosettes, 60 dwarf rosettes generally with sickle-shaped leaves, 275 *franciscana*-like rosettes of which 130 were discarded for lack of garden space, 95 rosettes of uncertain relationship at this stage of their development. As the culture matured many plants departed from the indications of their rosette stages, except that the plants with *franciscana*-like rosettes all developed into the *franciscana type*. This group of segregates even as half-grown rosettes may be selected with a high degree of certainty.

The group of 156 *Lamarckiana*-like rosettes produced 84 plants of *neo-Lamarckiana*, 37 plants *Lamarckiana*-like in foliage but with bud tips more *franciscana*-like and flowers of various sizes, 20 dwarfs *nanella*-like in foliage and bud characters, 1 plant of the *hero type*, 6 plants of the *franciscana dwarf type*, 8 plants died.

The 60 dwarf rosettes, generally with sickle-shaped leaves, developed 50 plants of the *franciscana dwarf type*, 10 plants died.

Of the 275 rosettes *franciscana*-like 130 were discarded for lack of garden space, 142 developed plants of the *franciscana type*, 3 died.

From the 95 rosettes of uncertain relationships there developed 2 plants of *neo-Lamarckiana*, 2 plants of the *hero type*, 61 plants of the *franciscana type*, 22 plants of the *franciscana dwarf type*, 8 plants died.

The composition of the F_6 generation presented the following distribution of plants:

86 plants of *neo-Lamarckiana*, 14.1 per cent.
37 plants *Lamarckiana*-like in foliage but with bud tips more like *franciscana*.
20 dwarfs, *nanella*-like in foliage and bud characters.

14 etiolated seedlings and rosettes, *albida*-like. These died early.
3 plants of the *hero type*.
333 plants of the *franciscana type* including 130 typical rosettes discarded for lack of garden space, 54.5 per cent.
78 plants of the *franciscana dwarf type*.
40 plants died, before their characters could be determined, 11 as seedlings or young rosettes and 29 older plants after the culture was set in the ground.

611 Total of germinations, 87.2 per cent.

It will be noted that the F_6 generation in the character of its segregates parallels the performance of the F_5 generation and is also the same as that of the F_4 except for the addition of *nanella*-like dwarfs. It is also interesting to note the percentages of the two largest groups in the three generations. *Neo-Lamarckiana* made up 5.4 per cent. of the F_4, 10.1 per cent. of the F_5, and 14.1 per cent. of the F_6. The *franciscana type* composed 43.3 per cent. of the F_4, 49.3 per cent. of the F_5, and 54.5 per cent. of the F_6. The percentages of *neo-Lamarckiana* in the F_4 and F_5 are probably lower than they should be because large numbers of *Lamarckiana*-like rosettes (48 in the F_4 and 18 in the F_5) failed to send up shoots and some of these rosettes may have been plants of *neo-Lamarckiana*. None of the percentages in the cultures can be more than approximate because of the large mortality which, however, was much higher in the F_4 and F_5 than in the F_6. Therefore it seems probable than the percentages of the F_6 are more nearly accurate.

Plant 19.41, II–2, representative of *neo-Lamarckiana*, was selected to be parent of the F_7 generation.

The F_7 Generation, and the Fourth Generation from *Neo-Lamarckiana*.

The F_7 generation, culture 20.41, was from the F_6 plant 19.41, II–2, the fourth plant to bear the name *neo-Lamarckiana* in this selfed line.

The culture was grown from earth-sown seeds in pans kept for 7 weeks. There were sown 494 seeds, the contents of 2 capsules, and during the 7 weeks the seedlings which came up and produced

rosettes numbered 224, a germination of 45.4 per cent. This was poor germination as contrasted with the percentages of 87.4 for the F_4, 82.5 for the F_5, and 87.2 for the F_6, from seeds experimentally forced to complete germination, and it became a matter of interest to see what classes of segregates would be represented in the culture. The results gave striking evidence for my contention that the earth-sown cultures of much of the work on *Œnothera* hybrids are likely to have shown only partially the variety and numbers of types represented by seeds that germinate slowly or not at all under the usual methods by which cultures are started.

All of the 224 rosettes lived and the composition of the F_7 generation from earth-sown seed was as follows:

59 plants of *neo-Lamarckiana.*
12 plants *Lamarckiana*-like in foliage but with bud tips more like *franciscana.*
5 plants remained as *Lamarckiana*-like rosettes.
147 plants of the *franciscana type.*
1 plant of the *franciscana dwarf type.*

224 Total of germinations in earth, 45.4 per cent.

Comparing this list of segregates with those of the 4th, 5th and 6th generations it is evident that the lower germination from earth-sown seeds cut out almost wholly the three groups of dwarfs, *e.g.*, the *albida*-like dwarf, the *nanella*-like forms, and the *franciscana dwarf type.* Assuming that the *neo-Lamarckiana* genotype had not changed, a full germination of 87 per cent. would have given about 430 plants. The representation of *neo-Lamarckiana* might have been about 14 per cent. of 430 or 60 plants and it happened that the number of plants of *neo-Lamarckiana* numbered 59. A full representation of the *franciscana type* might have been 50 per cent. of 430 or 215 plants and the actual number was only 147. Therefore the low germination probably also affected the representation of the *franciscana type.* There were no *hero* plants. The justification of this speculation will be shown by the performance of the succeeding F_8 generation which was grown from seeds forced to complete germination.

Plant 20.41–157 was selected from the F_7 as the representative of *neo-Lamarckiana* to be parent of the F_8 generation.

THE F_8 GENERATION, AND THE FIFTH GENERATION FROM *Neo-Lamarckiana*.

The F_8 generation, culture 21.41, from the F_7 plant 20.41–157, the fifth plant of *neo-Lamarckiana*, is of special interest as a culture grown from seeds forced to complete germination following a generation of earth-sown seeds. The contents of 2 capsules, 622 seeds, after soaking 24 hours were subjected while in water to alternate exhaust and pressure up to 30 pounds 7 times during the following 24 hours. There were 560 germinations within 4 days after this treatment and germination was completed after 12 days. Apparently an alternation of pressure with exhaust is particularly effective in forcing water through the seed coats. The seedling numbered 573, a germination of 92.1 per cent.

It was necessary to remove the seed coats from 16 germinating seeds. There were 9 seedlings of the sort that I have named *stumpy* because the hypocotyl fails to develop a root and consequently the seedling dies after a few days. Stumpy seedlings are not uncommon in *Œnothera* cultures but only recently have I made records of their presence. There were 13 etiolated *albida*-like seedlings which soon died. Other mortality reduced the culture to 537 plants when set in the field.

The rosettes were not grouped before planting so there is no record of their classification at this stage. This proved unfortunate since an unusual drought resulted early in a very high mortality in the field and I can only judge the probable effects of this drought on the groups most affected by the final counts of plants made during the summer.

A new type appeared in this generation which I shall refer to as *blunt tips*, distinguished as follows:

Blunt tips.

Rosettes. Small with narrow obtuse pointed leaves.

Mature plant. Short, erect with narrow leaves generally obtuse pointed.

Sepal tips. Short and blunt.

Flowers. Medium sized.

Capsules. Elongated, medium sized.

Pollen. Chiefly shriveled grains.

In habit of growth, foliage, and inflorescence this type resembles closely *O. oblonga* from *Lamarckiana* but the capsules were not short as in that plant.

The final composition of the F_8 generation presented the following distribution of plants:

72 plants of *neo-Lamarckiana*, 12.6 per cent.
39 plants *Lamarckiana*-like in foliage but with bud tips more like *franciscana*.
10 plants remained as *Lamarckiana*-like rosettes.
1 broad leaved dwarf rosette, *nanella*-like, failed to mature.
13 etiolated seedlings and rosettes, *albida*-like. These died early.
1 plant of the *hero type*.
1 plant, *blunt tips*.
171 plants of the *franciscana type*, 29.8 per cent.
24 plants remained as *franciscana*-like rosettes.
23 plants of the *franciscana dwarf type*.
218 plants died before their characters could be determined, 23 as seedling or young rosettes and 195 older plants in the field.

573 Total of germinations, 92.1 per cent.

Since the F_8 generation presents the same variety of segregates as appeared in the 4th, 5th and 6th generations, it is evident that the genotype of *neo-Lamarckiana* cannot have changed materially as carried along in this selfed line. It must also have been the same in the F_7 and the poor representation of segregates in that generation was undoubtedly due to incomplete germination of the earth-sown seeds as set forth in the preceding section.

The high mortality among the plants of the F_8 generation in the field probably affected chiefly the groups of dwarfs and to some extent the class of the *franciscana type*. The 72 plants of *neo-Lamarckiana* constituted 12.6 per cent. of the 573 seedlings, a percentage close to the figures 10.1 and 14.1 for the 5th and 6th generations. The 171 plants of the *franciscana type* together with the 24 *franciscana*-like rosettes constitute only 34 per cent. of the germinations which in the 4th, 5th and 6th generations was close to

50 per cent., so it seems probable that this class as in the F_7 suffered with the high mortality of the culture.

It is of particular interest that the group of *neo-Lamarckiana*, although not large in number, has proved to be least affected by the vicissitudes of seasons constituting from the 5th generation on from 10 to 18 per cent. of the cultures.

Plant 21.41–85 was selected from the F_8 as the representative of *neo-Lamarckiana* to be parent of the F_9 generation.

The F_9 Generation, and the Sixth Generation from *Neo-Lamarckiana.*

The F_9 generation, culture 23.41, was from the F_8 plant 21.41–85, the sixth plant of *neo-Lamarckiana* in this selfed line. From 2 capsules 553 seeds, after soaking 24 hours, were subjected while in water to alternate exhaust and pressure of 30 pounds 8 times during the following 12 hours. There were 496 germinations within 4 days and germination was found to be complete after 15 days. The seedlings numbered 512, a germination of 92.6 per cent.

Stumpy seedlings (those which failed to develop a root) numbered 3 and it was necessary to remove the seed coats from 14 germinating seeds. There were 23 etiolated *albida*-like seedlings which soon died. Other mortality reduced the culture to 442 plants when set in the field.

Three new types appeared in this generation, *pointed tips*, *bushy*, and *hairy*, with distinguishing characters as described below.

Pointed tips.

Rosettes. Small with acute pointed leaves.
Mature plants. Short, erect, with narrow leaves acute pointed.
Buds. Red sepals and reddish hypanthium.
Sepal tips. Attenuate and green.
Flowers. Medium sized.
Capsules. Elongated, fair yield of seeds.
Pollen. About 80 per cent. shriveled.

In habit of growth, foliage and acute pointed sepal tips this plant resembles *scintillans* from *Lamarckiana*. As with *scintillans* the low fertility is probably associated with irregularities of chromosome distribution.

Bushy type.

The mature plant is without long branches from the rosette and it develops in greater number and to much greater length those branches above. Otherwise the *bushy type* is quite the same as *neo-Lamarckiana*.

Hairy type.

Rosettes. Broad leaves.

Mature plants. Short, erect, broad leaves, stems with a very heavy pubescence.

Buds. Much more hairy than *franciscana*.

Sepal tips. Short, blunt, and very hairy.

Capsules. Medium length, fair yield of seeds.

Pollen. Chiefly shriveled.

I have never seen such remarkable pubescence in material derived from *biennis* and *franciscana*.

The final composition of the F_9 generation presented the following distribution of plants:

95 plants of *neo-Lamarckiana*, 18.5 per cent.
48 plants *Lamarckiana*-like in foliage but with bud tips more like *franciscana*.
24 plants remained as *Lamarckiana*-like rosettes.
23 etiolated seedlings and rosettes, *albida*-like. These died early.
5 plants of the *hero type*.
3 plants, *blunt tips*.
1 plant, *pointed tips*.
4 plants of the *bushy type*.
1 plant of the *hairy type*.
241 plants of the *franciscana type*, 47.1 per cent.
1 plant remained as a *franciscana*-like rosette.
11 plants of the *franciscana dwarf type*.
55 plants died before their characters could be determined, 48 as seedlings or young rosettes and 7 older plants in the field.

512 Total of germinations, 92.6 per cent.

The F_9 generation was in several respects of particular interest. The representation of *neo-Lamarckiana*, 18.5 per cent., was larger than in any previous generation. The ratio of the *franciscana type*, 47.1 per cent., was close to previous figures when germination was good and mortality low. Three new forms appeared which together with repetitions of previous types made this generation much more varied than any previous culture in this line.

Plant 23.41–335, representative of *neo-Lamarckiana* was selected to be parent of the F_{10} generation.

The F_{10} Generation, and the Seventh Generation from *Neo-Lamarckiana*.

The F_{10} generation, culture 24.41, from the F_9 plant 23.41–335, the seventh plant of *neo-Lamarckiana* in this selfed line, was the last generation grown. The contents of 2 capsules, 433 seeds, were soaked for 6 hours and then treated to alternate exhaust and pressure of 30 pounds five times in 24 hours. Within 4 days 365 seeds had germinated, and germination was complete after 12 days. There were 384 seedlings, a germination of 88.7 per cent.

Stumpy seedlings numbered 16 and it was necessary to remove the seed coats from 26 germinating seeds There were 19 *albida*-like seedlings and young rosettes which soon died. The culture contained 316 plants when set in the field.

The final composition of the F_{10} generation presented the following distribution of plants:

84 plants of *neo-Lamarckiana*, 21.9 per cent.
21 plants *Lamarckiana*-like in foliage but with bud tips more like *franciscana*.
19 etiolated seedlings and rosettes, *albida*-like. These died early.
2 dwarfs, *nanella*-like in foliage and bud characters.
1 plant of the *hero type*.
1 plant, *blunt tips*.
1 plant, *pointed tips*.
3 plants, *bushy type*.
185 plants of the *franciscana type*, 48.2 per cent.
13 plants of the *franciscana dwarf type*.

2 narrow-leaved dwarf rosettes failed to mature.
52 plants died before their characters could be determined, 49 as seedlings or young rosettes and 3 older plants in the field.

384 Total of germinations, 88.7 per cent.

The F_{16} generation gave again examples of *blunt tips*, *pointed tips*, and the *bushy type* which first appeared in the ninth generation. The representation of *neo-Lamarckiana* was 21.9 per cent., the highest percentage in the series of seven generations (Table VI.). The ratio of the *franciscana type*, 48.2 per cent., was again close to previous figures.

Crosses between *Biennis* and *Neo-Lamarckiana*.

My studies have concerned themselves chiefly with cultures from *neo-Lamarckiana* designed to establish by selection the genetype and to record its behavior through a number of generations. Crosses have, however, been made with *biennis* to determine whether *neo-Lamarckiana* would, like *Lamarckiana*, produce twin hybrids.

The cross *biennis* × *neo-Lamarckiana* was studied in two cultures (16.44 and 17.44) from seed forced to complete germination and both cultures presented two distinct classes of plants: (1) a type similar to *biennis* in flower structure and in having broad leaves and stems mostly without red papillæ, and (2) a type suggesting *franciscana* in its narrow leaves and stems bearing numerous red papillæ but unlike *franciscana* in having generally small flowers. The types were readily separated in the rosette stage (Fig. 9). Therefore, we have from this cross twin hybrids comparable to those from *biennis* × *Lamarckiana* and separated by differences of habit and foliage such as distinguish the twin hybrids *læta* and *velutina* of the latter cross (de Vries 1913). Flower size and structure did not follow a clear system, both classes presenting a wide range of variation. The distribution of the two classes is shown in Table I.

The reciprocal cross, *neo-Lamarckiana* × *biennis*, involving the same parent plants as the former cross, was likewise studied from two cultures (16.43 and 17.43) also grown from seed forced to complete germination. The seedlings started light green in color and

weak, and such rosettes and older plants as lived had light green leaves and never became strong. Only 5 plants (17.43) reached maturity and these had a habit and leaf form similar to *biennis*

TABLE I.

TWIN HYBRIDS FROM THE CROSS *biennis* × *neo-Lamarckiana*.

Class 1, plants *biennis*-like in habit and in having broad leaves and stems mostly without red papillæ.

Class 2, plants suggesting *franciscana* in having narrow leaves and stems with numerous red papillæ.

Culture.	Total Germination.	Percentage of Germination.	Plants in Class 1.	Plants in Class 2.	Died.
16.44	127	98.4	11	109	7
17.44	197	92.0	8	185	4

but with stems red papillate. Thus, as far as expressed, this cross gives a uniform progeny as does the cross *Lamarckiana* × *biennis*. The record of the cross is given in Table II.

TABLE II.

RECORDS FROM THE CROSS *neo-Lamarckiana* × *biennis*.

Culture.	Total Germination.	Percentage of Germination.	Living through the Summer as Rosettes.	Reaching Maturity.	Died.
16.43	101	84.1	24		77
17.43	215	97.3	15	5	195

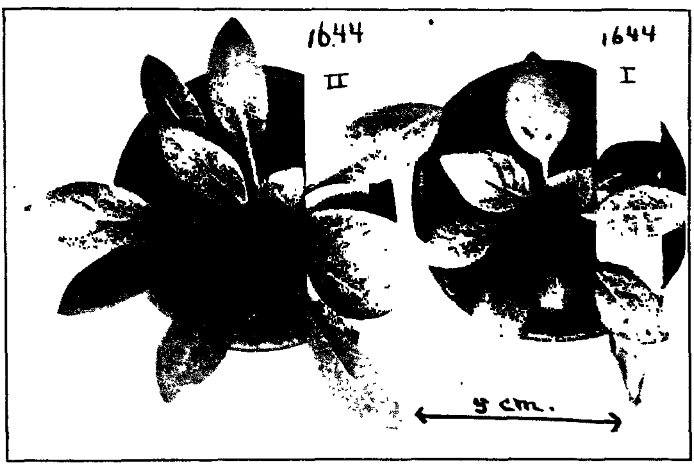

FIG. 9. Young rosettes of the cross *biennis* × *neo-Lamarckiana* (16.44), representative of the two classes similar to the twin hybrids *læta* and *velutina* from the cross *biennis* × *Lamarckiana*.

We find, therefore, in the crosses to *biennis* that *neo-Lamarckiana* as the pollen parent behaves in the same manner as does *Lamarckiana* to the same plant, producing twin hybrids comparable to *læta* and *velutina*. It will be remembered that this behavior of *Lamarckiana* is held by critics of the mutation theory of de Vries to be strong evidence that *Lamarckiana* forms two classes of fertile gametes and is therefore heterozygous. The similar behavior of *neo-Lamarckiana*, a known hybrid, gives substantial support to this criticism of the purity of *Lamarckiana*.

The Behavior, When Selfed, of Derivatives from *Neo-Lamarckiana*.

There is given below the behavior, when selfed, of derivatives from *neo-Lamarckiana* as listed in Table VI.

The franciscana type.

This form which always appears from *neo-Lamarckiana* in numbers close to 50 per cent. of the culture was tested from a selfed plant, through culture 24.42. The contents of one capsule, 482 seeds, on forced germination [2] gave 467 seedlings, a germination of 96.8 per cent. The rosettes were uniform and *franciscana*-like except for one narrow-leaved dwarf which on maturing proved to be a plant of *pointed tips*. The culture matured 423 plants all of the *franciscana type* except for the one *pointed tips*. The *franciscana type* may then be considered as breeding true and since it has almost perfect pollen output and almost complete seed fertility the type is probably close to homozygous.

The franciscana dwarf type.

This plant, like the *franciscana type*, has always been thrown by *neo-Lamarckiana*, but in much smaller numbers (see Table VI.), never above 13 per cent. of the culture. A selfed plant gave on forced germination culture 24.43 with 255 seedlings from 330 seeds, the contents of one capsule, a germination of 77.3 per cent. There matured 233 plants all of the *franciscana dwarf type*. The high

[2] Forced germination in this section of the paper means that the seeds after soaking in water for 6 hours were treated to alternate exhaust and pressure to 30 pounds, 5 times in 24 hours.

degree of seed fertility and almost perfect pollen together with this breeding test indicate that the *franciscana dwarf type* cannot be far from homozygous.

Albida-like.

This plant type similar to *albida* from *Lamarckiana* in having etiolated leaves on its small and weak seedlings and young rosettes has never matured, even when protected, and consequently has given me no seed.

Nanella-like.

This form, similar to *nanella* from *Lamarckiana* as rosettes, in dwarf habit and in foliage and bud tips, but with flowers of various size, was tested in culture 19.45 from a selfed plant. The contents of two capsules, 206 seeds, on forced germination gave 168 seedlings, a germination of 81.6 per cent. There matured 141 plants breeding true to the characters listed above. This true breeding together with the good seed fertility and good pollen development indicate a high degree of purity in the type.

The hero type.

As with other triploid plants of *Œnothera* the fertility of the *hero type* is very low and numerous selfings must be made to obtain seed. Thus, 26 selfings of a plant (23.41–404) gave only 177 seeds from 15 shriveled capsules and these on forced germination produced only 29 seedlings (Culture 24.45, Table III.). A large proportion of the pollen grains are four lobed, but almost all are shrunken and dead. Seed sterility is very high.

Cultures have been grown from five different plants with results as shown in Table III. The variation in the progeny was extraordinary but most of the plants were weak or dwarf and the mortality in the cultures was high. A few plants appeared of the *hero type*. One plant (20.43–3) presented 28 chromosomes, but its progeny (Culture 21.43, Table III.) although fairly large consisted of weak rosettes which failed to mature. Two plants on examination gave chromosome counts close to 21 and it seems safe to conclude that the *hero type* is generally triploid. It is obvious that the *hero type* is strongly heterozygous.

TABLE III.

Generations in Selfed Lines of the *hero type* from *Œnothera neo-Lamarckiana*.

Culture.	Parent Plant.	Seeds.	Seedlings.	*Hero type* Mature.	*Hero type* Rosettes.	Large Narrow-leaved Rosette.	Dwarfs Mature.	Dwarfs Rosettes.	Died.
19.43...	17.41, XXII-1	25 (4 capsules)	15	2		1	2		10
20.43...	19.43, I 3	81 (1 capsule) Earth sown	10	4	3			3	
21.43...	20.43-3 A plant with 28 chromosomes	342 (3 capsules)	209	151 rosettes showed a very great range in variation. No plants developed shoots and all of them died early or in the field.					209
24.44...	23.41-270	133 (18 capsules)	15	2			7		6
24.45...	23.41-404	177 (15 capsules)	29	8		1	4		16

Blunt tips.

The contents of one capsule, 256 seeds, on forced germination produced 241 seedlings, a germination of 94.1 per cent. The culture, 24.46, numbered 205 plants, when set in the field, of which all lived except 2 narrow-leaved dwarf rosettes. Of the 203 plants which matured only 9 were *blunt tips;* the remainder were *franciscana*-like.

It is clear that *blunt tips* is heterozygous and the reason why it produces so few of its kind is probably the high degree of sterility of its pollen. *Blunt tips* apparently develops very few gametes of its kind but many of the *franciscana type* which accounts for the large number of plants of the latter in its progeny.

Pointed tips.

This interesting plant has appeared only twice from *neo-Lamarckiana*, one plant in culture 23.41 and one plant in culture 24.41. It has, however, been thrown once by *franciscana*, culture 23.21, and is the only variant which *franciscana* has produced in the ten generations of this line which I have grown with its total of about 2,300 plants.

Plant 23.41–156 from *neo-Lamarckiana* produced only 28 seeds in two shriveled capsules and none of these germinated. I had better results from plant 23.21–165 thrown by *franciscana.* The contents of one capsule, 384 seeds, on forced germination gave 361 seedlings, a germination of 94 per cent. This culture, 24.25, matured 332 plants all *franciscana*-like except for 3 plants of *pointed tips.* Reciprocal crosses between plant 23.21–165 and *franciscana* gave in both cultures, 24.26 and 24.27, only *franciscana* except for 3 dwarfs. Since the pollen of pointed tips is 80 per cent. shriveled, it seems clear that this plant like *blunt tips* is heterozygous and develops very few gametes of its genotype but many of the *franciscana type* so that its progeny is almost wholly of the latter.

Bushy type.

The *bushy type*, which differs from *neo-Lamarckiana* only in its habit of growth, gave a progeny when selfed very much like that of the latter. Culture 24.48 started with 272 seedlings from the

forced germination of 295 seeds, the contents of one capsule, a germination of 92.2 per cent. which was close to that of *neo-Lamarckiana*. The final composition of the culture was:

29 plants of *neo-Lamarckiana*, } 25.3 per cent.
40 plants of *bushy type*, }
11 etiolated seedlings and rosettes, *albida*-like. These died early.
1 plant of the *hero type*.
1 plant of *hairy type*.
164 plants of the *franciscana type*, 60. 3 per cent.
13 plants of the *franciscana dwarf type*.
3 narrow-leaved dwarfs.
10 plants died before their characters could be determined.

272 Total of germinations, 92.2 per cent.

This performance was so close to that of *neo-Lamarckiana* in the character and proportions of the derivatives that one may well doubt whether *bushy type* is genetically distinct from this plant. It seems more probable that its peculiarities of habit represent only a modification under cultivation.

Hairy type.

The contents of one capsule, 216 seeds, on forced germination gave 168 seedlings, a germination of 77.7 per cent. The culture, 24.49, at maturity contained the following:

35 plants of the *hairy type*.
106 plants of the *franciscana type*.
12 plants of the *franciscana dwarf type*.
1 broad-leaved dwarf rosette.
1 linear-leaved dwarf rosette.
13 plants died before their characters could be determined.

168 Total of germinations, 77.7 per cent.

The *hairy type* is clearly heterozygous and as with *blunt tips* and *pointed tips* gives the *franciscana type* in large proportions. Its pollen is largely shriveled and it seems probable that this form of sterility may eliminate gametes which if produced would give higher proportions of the *hairy type*.

Discussion.

The body of this paper has shown that *Œnothera neo-Lamarckiana* through successive generations in a selfed line throws similar sets of new types, each type with its own range of frequency and all very different from one another. This is behavior parallel to the production by *Lamarckiana* of its particular set of "mutants." Any new type from *neo-Lamarckiana* is a mutant when it becomes clear that an active new gene is present in its genetical constitution or when a gene is lost or rendered impotent. It is easy to state such conditions for the proof of a mutation but quite another thing to establish evidence for a particular case. The present study does not attempt to analyze the genetical constitution of the new types from *neo-Lamarckiana.* It does, however, make clear that a hybrid *Œnothera* performs in a manner similar to that of *Lamarckiana* and thus thereby strongly indicates that *Lamarckiana* is itself heterozygous.

My studies have concerned themselves chiefly with cultures from *neo-Lamarckiana* designed to establish by selection the genotype and to record its behavior through a number of generations. It has become evident that the genotype is heterozygous and there is no evidence that selection has changed its character. The breeding behavior of *neo-Lamarckiana* in the seventh generation was quite the same as in earlier generations so far as the more common of its derivatives are concerned (Table VI.). *Neo-Lamarckiana* can be depended upon always to throw certain variants in fairly fixed proportions. Other variants are rare and appear only occasionally or in small numbers. When *biennis* is pollinated by *neo-Lamarckiana*, twin hybrids result. The derivatives from *neo-Lamarckiana* when selfed behave variously. Some are essentially homozygous as the *franciscana type*, the *franciscana dwarf type*, and *nanella*-like. Others are clearly heterozygous as the *hero type*, *blunt tips*, *pointed tips*, and the *hairy type*. Thus in various respects we see that *neo-Lamarckiana* and its derivatives behave in a manner similar to that of *Lamarckiana* and its "mutants."

Of the derivatives from *neo-Lamarckiana* some show wholly or in large part characters of segregates. Notable among these are the *franciscana type*, the *franciscana dwarf type*, and *nanella*-like. Others present peculiarities difficult to associate with either parent

as in *albida*-like, the *hero type*, *blunt tips*, *pointed tips* and the *hairy type*. These may be truly mutants although my studies do not prove them to be, for it is not always easy to establish a character as wholly new to the ancestry of any line of plants or animals. The highly heterozygous character of these plants and their poor breeding through high degrees of sterility make them very difficult for study. If they are mutants it is important to recognize the fact that they have been derived from the heterozygous or impure species *neo-Lamarckiana*.

A matter of particular interest in a comparison of *neo-Lamarckiana* with *Lamarckiana* is that of sterility both gametic and zygotic. The pollen grains of *Lamarckiana* are about 50 per cent. shriveled. The pollen of *neo-Lamarckiana* has been examined from the F_3 plant 15.53*a*, the F_4 plant 16.41, XIX–4, and the F_7 plant 20.41–157 (all plants being of the line described in this paper), and the results show uniformly 50 per cent. or somewhat more of shriveled grains. In this connection it is of interest to note that the parent species *biennis* has 50 per cent. or more of shriveled pollen while that of *franciscana* is almost wholly perfect. *Lamarckiana* and *neo-Lamarckiana*, therefore, agree in showing about the same degree of pollen abortion.

With respect to seed sterility, however, *Lamarckiana* and *neo-Lamarckiana* present sharp contrasts probably of important genetical significance. I have a five-year record of the seed fertility in a line of *Lamarckiana* which I am carrying and which originally came from seed of de Vries. The results of cultures from seeds of this line, forced to complete germination, is given in Table IV. and agree closely with the findings of de Vries and others. *Œnothera Lamarckiana* has a seed fertility, differing somewhat in various lines, but apparently rather close to 35 per cent.

TABLE IV.

SEED FERTILITY IN A LINE OF *Œnothera Lamarckiana*.

Culture *Lamarckiana* M.	Parent Plant.	Seeds Sown.	Seedlings.	Percentage of Germination.
15.10	14.10*a*	701 (2 capsules)	226	32.2
16.10	15.10*a*	226 (1 capsule)	96	36.1
17.10	16.10*a*	550 (2 capsules)	198	36.0
19.10	17.10*a*	102 (1 capsule)	40	39.2
24.10	23.10–4	254 (1 capsule)	94	37.0

The seed fertility of *neo-Lamarckiana* is high, about 90 per cent. My record for the line in cultures forced to complete germination is given in Table V. where are also included data from certain earth-sown cultures in which the percentage falls far below the proper figure (see cultures 13.35, 15.53 and 20.41), showing that earth-sown cultures of this material must give only a partial expression of its possibilities.

TABLE V.

SEED FERTILITY IN A LINE OF *Œnothera neo-Lamarckiana.*

Culture.	Parent Plant.	Seeds Sown.	Seedlings.	Percentage of Germination.	
F_1 13.35	*franciscana* × *biennis*	652 (4 capsules)	328	50.5	Earth sown
F_2 14.53	13.35*c*	454 (5 capsules)	387	85.2	Earth sown
F_3 15.53	14.53*c*	625 (4 capsules)	291	46.6	Earth sown
F_4 16.41	15.53*a* 1st *neo-Lamarckiana*	764 (3 capsules)	668	87.4	
F_5 17.41	16.41, XIX–4 2d *neo-Lamarckiana*	1089 (5 capsules)	899	82.5	
F_6 19.41.	17.41, XVIII–2 3d *neo-Lamarckiana*	701 (3 capsules)	611	87.2	
F_7 20.41.	19.41, II–2 4th *neo-Lamarckiana*	494 (2 capsules)	224	45.4	Earth sown
F_8 21.41	20.41–157 5th *neo-Lamarckiana*	622 (2 capsules)	573	92.1	
F_9 23.41.	21.41–85 6th *neo-Lamarckiana*	553 (2 capsules)	512	92.6	
F_{10} 24.41.	23.41–335 7th *neo-Lamarckiana*	433 (2 capsules)	384	88.7	

It will be remembered that *neo-Lamarckiana* throws a large group, the *franciscana type*, constituting about 50 per cent. of the cultures and also a numerous group, the *franciscana dwarf type*. *Lamarckiana* presents no groups of derivatives comparable to these, its "mutants" being in small proportions as is true of the various other forms thrown by *neo-Lamarckiana*. A correlation is at once suggested between the high seed fertility of *neo-Lamarckiana*, about 90 per cent., and the appearance of the *franciscana type* and the *franciscana dwarf type*. Conditions in *neo-Lamarckiana* rendering sterile all or almost all of the seeds representing these two classes would give to *neo-Lamarckiana* a seed fertility below 40 per cent., a figure close to the seed fertility of *Lamarckiana*.

Is there any "mutant" of *Lamarckiana* which as the *franciscana* *type* from *neo-Lamarckiana* may suggest a parental form? Of its "mutants" *rubrinervis* and *erythrina* are plants most similar to wild œnotheras in habit of growth, narrow leaves, and red pigmentation; they have the appearance of weediness well known to American botanists, an appearance not shown by such forms as *scintillans*, *oblonga*, *nanella*, *gigas* and others. If one were to make a guess from the offspring of *Lamarckiana* which were most likely to resemble a putative parent ancestor, these plants would be selected because they have rather closely the characters and appearance of certain American wild species. It should be remembered that *rubrinervis* and *erythrina* are similar except for the brittleness of the stems of *rubrinervis*. Both plants come out rather rarely in cultures of *Lamarckiana* but may this not be due to the high seed sterility of this species? In short, may not these plants be representative of types resembling a parent of *Lamarckiana*, but a type which rarely appears because suppressed by conditions that determine the low seed fertility of *Lamarckiana?*

The very large group of *franciscana*-like plants appearing with every generation of *neo-Lamarckiana* may be thought of as reversion towards one of the parents of the cross. But, it should particularly be noted that no forms approaching the *biennis* parent have been thrown in the line from *neo-Lamarckiana*. Such forms should be plants having broad leaves, green stems, a flat-topped inflorescence, attenuate sepal tips, and medium-sized flowers with stigma below the tips of the anthers (autogamous). It is true, however, that a group of 14 green-stemmed plants with small flowers appeared in the F_2 from a plant 13.35*a*, sister plant to 13.35*c* of the line which gave *neo-Lamarckiana* (Davis 1916*a*, p. 230).

Two types with characters of *biennis* have been reported by van Overeem (1922) from a line of *Lamarckiana*. The first, *de Vriesii*, was a plant with 15 chromosomes derived from *Lamarckiana semi-gigas*. The second with 14 chromosomes came from *de Vriesii* and was named *bienniformis*. Both plants resembled *biennis* in foliage, inflorescence, buds and flower structure. Both had, however, red papillæ over the stems, a character, as far as I know, never present in *O. biennis*. These findings of van Overeem are of

interest as indicating some *biennis*-like ancestor in the history of *Lamarckiana*. It is noteworthy that *de Vriesii* should have come out of *Lamarckiana semi-gigas*, a triploid plant, from which one may expect unusual segregation phenomena in the maturation of gametes.

The fact that *neo-Lamarckiana* has in many respects the systematic characters of *Lamarckiana* is, of course, more a matter of horticultural and taxonomic interest than of genetical significance. It is clear from its breeding that *neo-Lamarckiana* is not close to the genotype of *Lamarckiana*, but the similarity of its behavior *in kind* indicates that *Lamarckiana* is an impure species and its "mutants" the products of a heterozygous germ plasm. There is value in this demonstration of the origin of a large-flowered vigorous garden product since it shows how easily *Lamarckiana* might have arisen as a hybrid out of the complex of *Œnothera* material present in Europe in the earlier part of the last century (Davis 1913).

Summary.

In Table VI. there is presented the record of *Œnothera neo-Lamarckiana* in selfed line through the seven generations that have been grown and from this record and other data presented in this paper the following conclusions seem evident:

TABLE VI.

Generations in the Selfed Line of *Œnothera neo-Lamarckiana* Isolated in the F_3 of a Line from the Cross *franciscana* $\times$ *biennis*.

Generation.	Total Germination.	Percentage of Germination.	*neo-Lamarckiana.*	Percentage of *neo-Lamarckiana.*	*franciscana type.*	Percentage of *franciscana type.*	*franciscana dwarf type.*	*albida-like.*	*nanella-like.*	*hero type.*	*blunt tips.*	*pointed tips.*	*bushy type.*	*hairy type.*	*Lamarckiana*-like in Foliage. Bud Tips more like *franciscana.*	Plants remaining as *Lamarckiana*-like Rosettes.
F_4 16.41	608	87.4	30	5.4	289	43.3	78	6							65	48
F_5 17.41	899	82.5	91	10.1	443	49.3	53	34	6	22					102	18
F_6 19.41	611	87.2	86	14.1	333	54.5	78	14	20	3					37	
F_7 20.41	224	45.4	59		147		1		From earth sown seeds						12	5
F_8 21.41	573	92.1	72	12.6	171	29.8	23	13	1 rosette	1	1				39	10
F_9 23.41	512	92.6	95	18.5	241	47.1	11	23		5	3	1	4	1	48	24
F_{10} 24.41	384	88.7	84	21.9	185	48.2	13	19	2	1	1	1	3		21	

1. With respect to the genotype *neo-Lamarckiana* no changes have been noted in the appearance of its characters from generation to generation. The figures in Table VI. suggest an increasing percentage of these plants as the generations followed one another, but it is not safe to draw conclusions since in every culture there was mortality and there were also numbers of *Lamarckiana*-like rosettes which failed to produce shoots and we cannot know to what extent ratios may have been modified by these facts. It is clear that in *neo-Lamarckiana* we have a representative of an impure species breeding true through a certain proportion of its progeny while obviously producing other types of viable gametes than those which in union with one another reproduce the parent *neo-Lamarckiana*. The plant is, therefore, heterozygous and the type has not been fixed by isolation through selfing, nor is there any reason to believe that it ever would become homozygous. Thus as an impure species *neo-Lamarckiana* may properly be compared with *Lamarckiana*.

2. *Neo-Lamarckiana* through successive generations always throws variants. Some of these have always been present when a culture fully expressed itself; such are the *franciscana type*, the *franciscana dwarf type*, *albida*-like, and the *hero type*. Other variants, appearing in small proportions, have not always been found in cultures which generally numbered under 500 plants; in this group are *nanella*-like, *blunt tips*, *pointed tips*, *bushy type*, and *hairy type*. In the nature of this behavior *neo-Lamarckiana* parallels that of *Lamarckiana*.

3. Unlike *Lamarckiana* my *neo-Lamarckiana* produces one variant in large proportions. This is the *franciscana type* which has generally made up close to 50 per cent. of the cultures. Thus we have in *neo-Lamarckiana* an impure species which combines the behavior of *Lamarckiana* in throwing certain variants in small proportions with a special peculiarity of producing in large proportions a type similar to one of its parents. This peculiarity seems to be correlated with the much higher degree of seed fertility of *neo-Lamarckiana*.

4. Every generation from *neo-Lamarckiana* has shown a rather large assemblage of plants *Lamarckiana*-like in foliage but with bud tips more like *franciscana*. This group has appeared in some-

what smaller proportions in later generations but it is not safe to conclude that these facts indicate a change in the behavior of *neo-Lamarckiana.*

5. The cross *biennis* × *neo-Lamarckiana* gives twin hybrids: (1) a type similar to *biennis*, and (2) a type suggesting *franciscana* in having narrow leaves. This is behavior similar to that of the cross *biennis* × *Lamarckiana* resulting in the twin hybrids, *læta* and *velutina.*

6. The cross *neo-Lamarckiana* × *biennis* gives a uniform progeny but the plants are light green and rarely live to maturity. In the fact that the progeny is uniform this behavior agrees with the cross *Lamarckiana* × *biennis.*

7. Some of the derivatives from *neo-Lamarckiana* breed true except for rare variants and these forms have good pollen and high seed fertility. They are the *franciscana type*, the *franciscana dwarf type*, and *nanella*-like, and may be considered as close to homozygous. These forms in many characters appear to be segregates.

8. Other derivatives are clearly heterozygous, breeding not at all true and exhibiting very large proportions of shriveled pollen and high degrees of seed sterility. Among these the *hero type* is apparently triploid with 21 chromosomes, one plant appearing with 28 chromosomes. Other forms, *blunt tips*, *pointed tips*, and the *hairy type*, reproduce themselves in very small proportions but throw large groups of the *franciscana* type with occasional other forms.

9. The derivative, *albida*-like, is etiolated and too weak to live.

10. An interesting plant, *bushy type*, easily recognized by its habit of growth, gives a progeny similar to that of *neo-Lamarckiana* and is probably not genetically distinct from the latter, its peculiarities representing a modification under cultivation.

11. The pollen of *neo-Lamarckiana* consists of about 50 per cent. shriveled grains as is also true of *Lamarckiana.*

12. The seed of *neo-Lamarckiana* is about 90 per cent. fertile in sharp contrast to that of *Lamarckiana* which is only about 35 per cent. fertile (varying with different lines). It seems probable that the high seed fertility of *neo-Lamarckiana* is correlated with the appearance of the large class of the *franciscana type*, constituting

about 50 per cent. of its progeny, together with the smaller group of the *franciscana dwarf type.*

With this paper I am closing the line of studies on *neo-Lamarckiana* and its derivatives. The ramifications of such studies might be followed indefinitely but I do not think that their findings would justify the work necessary and there are other subjects of *Œnothera* investigation that seem to me of greater interest. This study has reached the conclusion which was in my mind when the problem shaped itself some twelve years ago. They have shown (1) How an obviously impure species, *Œnothera neo-Lamarckiana,* may arise; (2) That such an impure species may maintain a constant condition of heterozygosis of the same genetical constitution although rigorously selected to its type; (3) That it throws with each generation similar sets of derivatives in fairly fixed proportions; (4) That some of its derivatives have characters of segregates and breed true, and that others are heterozygous; (5) That conditions of sterility suggest probable reasons for various vagaries of breeding behavior. In the various points of behavior outlived above *neo-Lamarckiana* and *Lamarckiana* agree.

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PNEUMONIA IN PITTSBURGH.

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(*Read April 24, 1924.*)

I. Introduction.

1. Incident to a general statistical study of the epidemiology of pneumonia begun by Dr. Tomanek, Fellow of the International Health Board, under the supervision of Dr. M. J. Rosenau and continued in association with Professor Wilson, it became early evident that statistically speaking the figures for the death rate from pneumonia in Pittsburgh would not fit into homogeneous series with those from other large cities. That is to say, if the frequency distribution of the pneumonia death rate in large cities is set up and the statistical constants of the distribution (mean, mode, standard deviation, skewness, and kurtosis) are determined the death rate for Pittsburgh is so high that it has such a major effect in the determination of the values of some of these constants that the statistical description of the frequency distribution is markedly different according as Pittsburgh is or is not included in the list of cities.

2. For example with the data for 1922:

Pneumonia Death Rate per 100,000 in Cities over 100,000.

R	10	30	50	70	90	110	130	150	170	190	*	290
F	1	1	4	10	12	11	12	6	3	1	*	1

The rates R advance by 20's and the frequencies F are the number of cities reported. Pittsburgh exceeds the nearest city by 100. The statistical constants with and without Pittsburgh are:

	Without Pittsburgh.	With Pittsburgh.
Mean	103.8 $\pm$ 4.5	106.7 $\pm$ 5.2
Median	100.9	101.8
Standard deviation	36.1 $\pm$ 3.1	42.0 $\pm$ 6.0
Skewness χ	.02 $\pm$ .10	.31 $\pm$ .10
$\beta_1 = \mu_3^2/\mu_2^3$	.003	1.315
$\beta_2 = \mu_4/\mu_2^2$	3.60	7.14
Mode	103.1	93.7

(For method of calculation see R. Pearl, "Biometry and Vital Statistics," Chap. XIII.) When Pittsburgh is rejected the curve is essentially normal; when it is retained there is a decided skewness and a high degree of leptokurtosis. Although the number of cities (66) is small so that the constants have large standard deviations, a large degree of statistically significant difference between two cases can be inferred from the persistence of similar differences year after year,[1] which in effect increases the number of data. Another test is found in the simple comparison of the deviation Pittsburgh — mean = 180 with the standard deviation of 42 or 36 which shows that the deviation is from 4 to 5 times the standard and should not occur once in 66 times, nor once in 10,000 times, if we use the ordinary table for computing chances.

3. When one meets such a condition of statistical instability due to a few outstanding observations several courses are open: (1) It is possible to appeal to the argument of the "huge error" and simply discard the anomalous figure. This simple if somewhat cavalier way out of the difficulty, though often justifiable in a case of a single upsetting observation, is hardly satisfactory when the same anomaly persists year after year. (2) One may adduce the argument of exceptional circumstances which in this case would be to point out the smoky nature of the Pittsburgh air, the intense industrialization of the Pittsburgh district, the heavy work and high temperatures to which labor in the steel industry is exposed, etc. There are however other smoky cities, other highly industrialized districts, other centers of manufacture in the iron and steel trades; and the solution is not to be found so much in the dismission of Pittsburgh for the general arguments thus adduced as in the determination of how much of the anomalous situation relative to pneumonia there is attributable to each of the factors and whether taken together they will account for the whole. Such a study would take time and much money directed

[1] In 1920 which showed a recrudescence of influenza we have

R	90	110	130	150	170	190	210	230	250	270	*	390
F	2	9	7	10	8	17	6	3	1	2	*	1

and Pittsburgh leads by 120. The mean for 1920 was 185 and the Standard deviation was 52, Pittsburgh departing from the mean by 4 times the standard deviation.

toward the quantitative measure of the various factors concerned and would depend for any chance of real success upon the active coöperation of the employment, welfare, and medical departments of the industry itself in assembling morbidity and other statistics which are not now kept. (3) It is possible to analyze in detail the statistical material available to determine whether there is internal evidence that some at any rate of the excessive number of deaths reported as due to pneumonia in Pittsburgh may not be due to other causes. It is only this relatively simple problem that we here attack. We desire to present the facts so far as we have been able to collect and digest them.

4. *Crude Pneumonia Death Rate, 1873–1923.*—So far back as mortality statistics are available two things are clear: first that the rate from pneumonia in Pittsburgh is high, second that it is getting higher. The mean number of deaths from pneumonia per 100,000 population, as reported by the city officials and city health department, during each of the past five decades are

1873–82	1883–92	1893–02	1903–12	1913–22
139.2	204.0	215.7	253.4	343.2

Omitting the year 1918 when the influenza epidemic upset the normal pneumonia figures throughout the whole country, the lowest rate for Pittsburgh was 99 in 1875 and the highest was 381 in 1920. The last annual figures that we have were kindly supplied by Dr. C. J. Vaux, Director of the Pittsburgh Department of Health, who under date of Feb. 13, 1924, writes "... for the year 1923: Deaths (all causes), 9,638. Rate per M. 15.6 (including non-resident deaths of 1,433). Violent deaths, 766. Pneumonia, all forms, 2,297." It is noteworthy that for a city with a shifting population and a large number (15 per cent.) of non-resident deaths the general death rate of 15.6 is not especially high;[2] that the violent deaths are 8 per cent. of the total; and that pneumonia as reported gives a proportionate mortality of 24 per cent. and a specific death rate of 380 for 1923.

[2] Adjustment from crude to standardized rate would raise the figure to about 16.6; the Pittsburgh rate is high, higher than it should be even with all allowances, but with proper allowances the general death rate does not appear to be inordinately high as is the case with the specific death rate from pneumonia.

practically identical with its 1920 maximum, highest of all large cities in the Union, and from 2 to 3 times the mean, median, or modal rate for all large cities.

II. Details with Regard to the Years 1900–1920.

5. Our registration area in this country has been steadily expanding from a relatively small number of states in 1880 to a relatively large number today. Contemporaneously there has probably been a considerable increase in the reliability and comparability of the material. By the year 1900 a large area was already available and pneumonia had become subdivided into lobar and broncho pneumonia. It is believed that the period 1900–1920 is on the whole the best in which to study in detail the general situation with respect to mortality from pneumonia and the particular situation in Pittsburgh. For this period the mortality statistics for pneumonia as supplied to us by the City Health Department of Pittsburgh can be subdivided and tabulated in narrow age groupings. The physician signing the death certificate is supposed to report as pneumonia every case of lobar pneumonia (Fränkel-Weichselbaum, type I.–IV.), the bacillary pneumonia of Friedländer, bronchitis capillaris of little children, atypical pneumonia, etc. Any pneumonia, lobular or lobar, which terminates a disease such as measles, whooping cough, scarlet fever, organic diseases of the heart, or the degenerative diseases, is supposed to be stated as a secondary cause of death. The multiplicity of causes of pneumonia, its various forms, the wide range of the clinical picture on the one hand, and on the other the limitations of the allowable nomenclature of pneumonia in the death certificate are the large and probably the main sources of unreliability and inaccuracy in pneumonia statistics. Although we give the figures for the subdivisions of pneumonia, it seems reasonable to expect that those in the whole group "pneumonia all forms" have a higher degree of trustworthiness and significance and should be used as a primary basis for the study.

6. The course of the crude death rate from pneumonia in Pittsburgh for the 21 years 1900–1920 is given in Table I. and Fig. 1.

TABLE I (See Fig. 1.).

CRUDE DEATH RATE FROM PNEUMONIA IN PITTSBURGH 1900–1920 PER 100,000.

Year.	Bronchopneumonia	Lobar and Undefined.	Pneumonia, All Forms.
1900	19	144	163
1	35	141	176
2	35	184	219
3	32	165	197
4	36	142	178
5	34	156	190
6	54	124	178
7	83	106	189
8	109	133	242
9	112	142	254
10	139	186	325
11	98	111	209
12	108	156	264
13	118	170	288
14	104	141	245
15	100	150	250
16	107	232	339
17	103	262	365
18	195	696 235*	893 430*
19	123	208	331
20	128	215	343

* Mean values of the year 1917 and the year 1919 for lobar.

The synopsis of Table I. with reference to extreme values, means, standard deviations and yearly trends is as follows: [3]

Pneumonia All Forms: Range between 163 in 1900 and 365 in 1917, (893 in 1918, influenza year). Mean = 256 ± 12, S.D. = 56 ± 8, Trend = + 10.

Bronchopneumonia: Range between 19 in 1900 and 195 in 1918 (influenza year). Mean = 89.2 ± 9.6, S.D. = 44, Trend = + 6.

Pneumonia Lobar and Undefined: Range 106 in 1907 and 262 in 1917, (696 in 1918). Mean = 16.7 ± 9., S.D. = 41, Trend = + 4.

Bronchitis: Range between 17 in 1920 and 60 in 1908. Mean = 35 ± 2., S.D. = 9, Trend = — .8.

For comparison Fig. 2 [4] gives for the ten original registration

[3] In computation of the mean, trend, and dispersion the influence of the very high data for pneumonia in the influenza year (1918) has been eliminated by using instead of the reported value for 1918 the mean value of the years 1917 and 1919.

[4] The figures throughout this article are on semi-log or ratio paper with the logarithmic scale vertical. This not only enables us to put into a graph

States of 1900 the course of the diseases shown in Fig. 1 for Pittsburgh. In both the exceptional conditions of the year 1918 disturb the course of the graphs; but it is clear at a glance that, whereas in

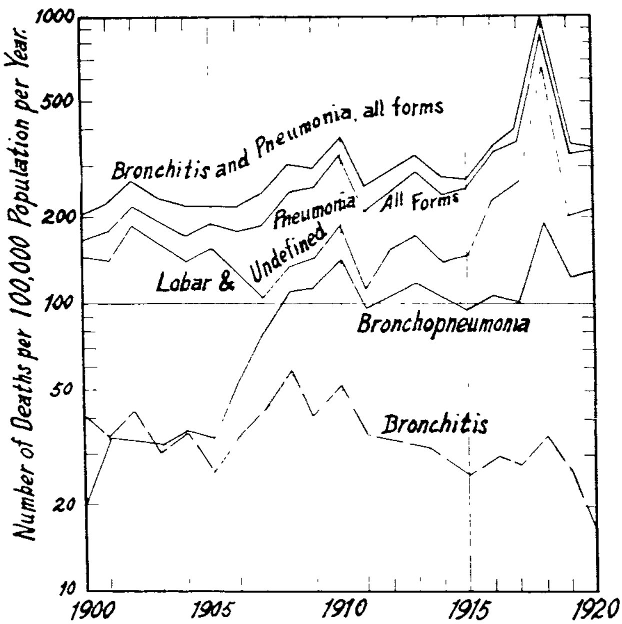

FIG. 1. Specific death rate from Pneumonia and Bronchitis in Pittsburgh, Pa., during the period 1900–1920.

Pittsburgh pneumonia, all forms, starts at about the same figure as, or a trifle lower than, that for the ten states, the curve for Pittsburgh climbs at a fairly steady rate to around 350, about doubling in 20 years, whereas for the ten states there is a slight tendency toward reduction. The calculated trend for Pittsburgh was + 10 per hundred thousand per annum while that for the ten states is — 0.4.[5] It is not only the present high mortality from pneumonia in Pitts-

great ranges of rates, as from 10 to 1,000, but shows at a glance the relative magnitudes of the variations and trends, for the variations from 900 to 1,000 and from 90 to 100 are pictured as of the same amount.

[5] A brief discussion of variation of pneumonia mortality in the ten original registration states of 1900 during the period 1900–1920 is given by us in *Proc. Nat. Acad. Sci., Washington*, Vol. 10, May, 1924, pp. 161–166.

burgh which is anomalous, but the steady increase during 21 years from a figure about normal, while in most places whatever progressive tendency there has been seems to have been toward slightly lower figures.

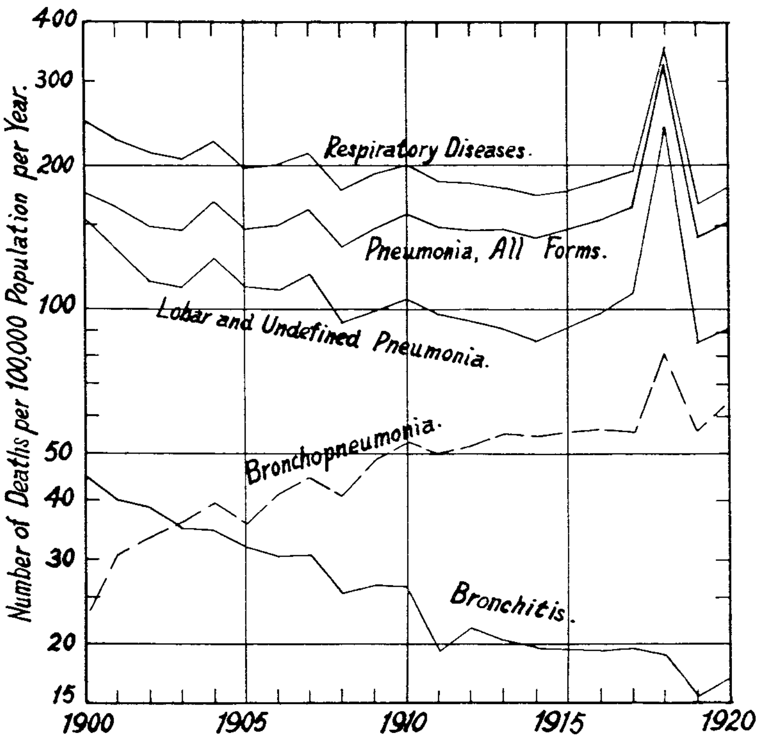

FIG. 2. Specific death rate from some respiratory diseases in the Ten Original Registration States, during the period 1900–1920.

7. Although the crude rates give a general idea of the sweep of the mortality in time, the different age distributions of different populations make it desirable to analyze the figures specifically by age. The figures for 1900, 1910, 1920 are taken from the U. S. Census; the intercensal years are figured by arithmetic interpolation.

Figure 4 gives for the ten original registration states graphs similar to those in Fig. 3 for Pittsburgh.

DISTRIBUTION OF THE POPULATION IN PITTSBURGH BY AGE.[6]

Age Group.	1900. Pop.	%	1910. Pop.	%	1920. Pop.	%
(0–1	11,152	2.5	12,578	2.4	12,018	2.0)
0–4	50,922	11.3	57,788	10.8	62,006	10.5
5–9	47,681	10.6	48,803	9.2	57,764	9.8
10–19	83,955	18.6	97,195	18.2	100,791	17.1
20–29	99,180	22.0	114,916	4.5	111,423	18.9
30–39	75,493	16.7	91,504	17.1	100,429	17.1
40–49	47,141	10.5	61,316	11.5	74,624	12.7
50–59	27,260	6.1	35,166	6.6	46,410	7.9
60–69	13,407	3.0	17,949	3.4	23,622	4.0
70–up	6,045	1.3	7,601	1.4	10,693	1.8
(unknown	428	0.1	1,853	0.3	581	0.1)
Total	451,512		533,905		588,343	

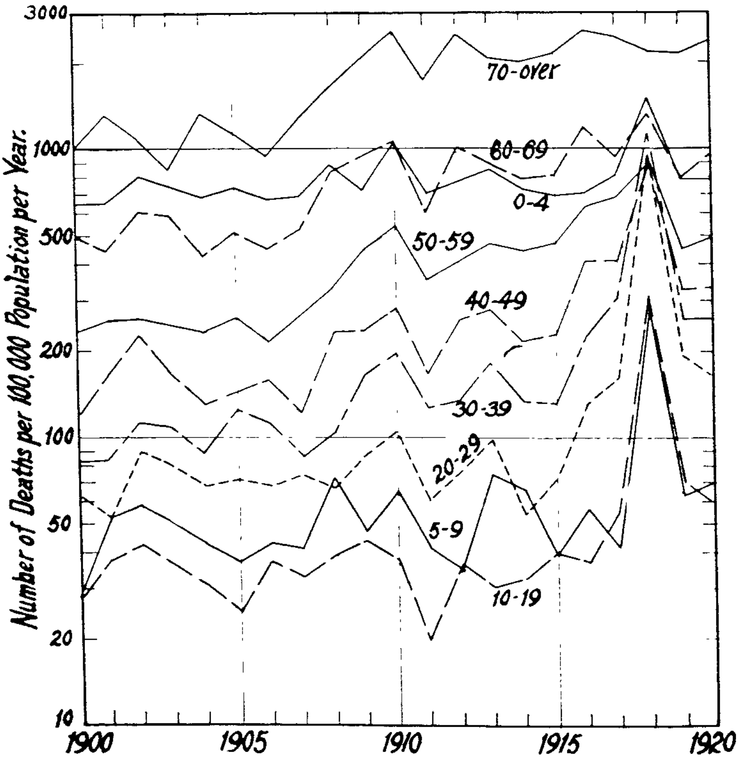

FIG. 3. Age specific death rate from Penumonia (all forms) in Pittsburgh, Pa., during the period 1900–1920.

[6] It is noticeable that the population of Pittsburgh is growing older, the percentages in the distribution decreasing below and increasing above an age

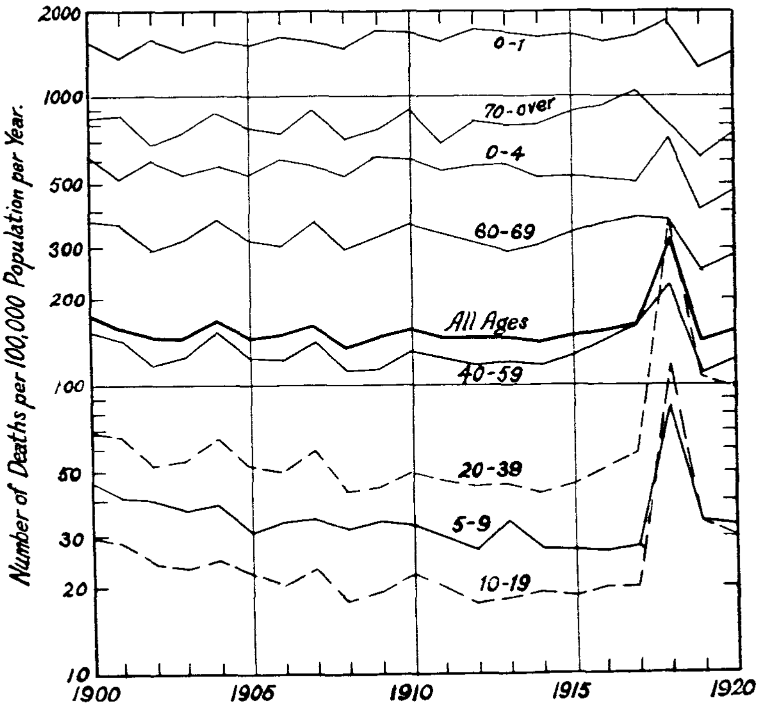

FIG. 4. Age specific death rate from Pneumonia (all forms) in the Ten Original Registration States, during the period 1900–1920.

NOTE. In the tables for the specific death rates the numbers are carried to one decimal place as is often the case; but the number of significant figures is variable. If N is the population and n the number of deaths, the rate p is n/N. The fluctuation of sampling for the number n is $\sqrt{(Npq)}$ where $q = 1 - p$ is approximately 1, so that approximately the fluctuation is $\sqrt{(Np)} = \sqrt{n}$. The fluctuation in the rate is $\sqrt{(pq/N)}$, approximately $\sqrt{(p/N)}$. As the rate p is given in hundreds of thousands, the population should be measured in hundreds of thousands. Thus in 1900 the population of age 0–4 is 51,000 and $N = 1/2$ in hundreds of thousands; the fluctuation in p expressed as 649.6 per hundred thousand is $\sqrt{(650/0.5)} = \sqrt{1{,}300} = 36$. We should therefore write 650 ± 36. In like manner in the group 70-up in 1900 we find $N = .06$ and the fluctuation is $\sqrt{(993/.06)} = \sqrt{16{,}200} = 127$ and the rate should be written 993 ± 127. On the other hand for the group 20–29 in 1900, $N = .99$ and $\sqrt{(64.5/.99)} = \sqrt{65.1} = 8.1$ and the rate is 64.5 ± 8.1. Thus

of about 40 years. Various refinements could have been applied in estimating the population in the different age groups in intercensal years in place of the crude method of arithmetic interpolation, but they seemed hardly worth while.

TABLE II.

AGE SPECIFIC DEATH RATE PER 100,000 POPULATION IN PITTSBURGH, 1900–1920.

A. Pneumonia, All Forms. (Fig. 3.)

Ages	1900.	1901.	1902.	1903.	1904.	1905.	1906.
0–4	649.6	657.0	817.0	758.0	697.4	735.0	674.0
5–9	27.3	54.4	58.4	51.9	43.7	37.3	43.5
10–19	27.4	38.7	42.6	36.4	31.1	25.4	37.0
20–29	64.5	53.8	89.9	80.8	68.2	72.0	68.2
30–39	83.4	84.2	113.1	108.7	89.3	121.2	111.7
40–49	114.4	164 4	228.0	166.7	130.7	145.4	156.8
50–59	231.0	254.1	256.8	245.9	233.0	215.0	267.4
60–69	499.8	442.2	607.9	597.0	427.1	511.3	459.0
70 up	993.2	1308.0	1091.5	858.0	1336.0	1157.5	959.0
	1907.	1908.	1909.	1910.	1911.	1912.	1913.
0–4	695.0	883.0	724.0	1056.0	707.0	783.0	855.0
5–9	41.2	74.0	47.3	65.4	42.3	35.6	75.7
10–19	33.2	39.2	43.8	38.0	19.8	36.8	30.5
20–29	74.2	67.7	87.3	106.3	61.0	76.3	99.9
30–39	87.8	104.2	160.5	197.6	127.3	135.0	183.5
40–49	120.0	232.5	235.8	280.6	165.6	257.0	279.7
50–59	267.4	327.5	449.0	542.0	353.4	412.2	473.2
60–69	520.0	827.0	957.0	1042.0	614.0	1002.0	897.2
70 up	1275.0	1687.0	2090.0	2580.0	1777.0	2500.0	2074.0
	1914.	1915.	1916.	1917.	1918.	1919.	1920.
0–4	735.5	693.0	711.0	814.0	1529 0	794.0	799.0
5–9	66.8	39.5	57.2	41.7	286.5	63.3	70.9
10–19	32.8	39.5	37.2	54.1	308.0	70.9	59.5
20–29	54.5	72.5	130 3	160.4	933.2	194.2	164.9
30–39	134.7	131.6	220.7	309.8	1156.0	263.2	262.7
40–49	219.0	226.5	414.0	410.6	890.0	327.0	333.7
50–59	447.0	470.0	630.0	682.9	905.0	452.0	489.0
60–69	800.0	812.0	1198.0	949.0	1320.0	808.0	978.0
70 up	2006.0	2132.0	2580.0	2467.0	2190.0	2126.0	2396.0

although the rates 2,126.0 and 2,396.0 for 1919 and 1920 in the age group 70–up look very different each is subject to a standard deviation of sampling of about 145, their difference 270 is subject to a fluctuation of about 200 and possesses no very great statistical significance. It would be more rational and more convenient to suppress the non-significant figures and round the numbers off, but as this procedure is not widespread in vital statistics we leave the figures as they stand. It should be remembered that few of the rates are good to 5 per cent. and many of them are not good to 10 per cent. Calculations such as these can safely be made with a 20–inch slide rule or even with a 10–inch rule and many of them throughout this paper have been so made. So much for fluctuations of simple sampling. The figures of the local authorities and of the federal authorities differ one from another, sometimes markedly, and even the federal figures printed in one and the same volume

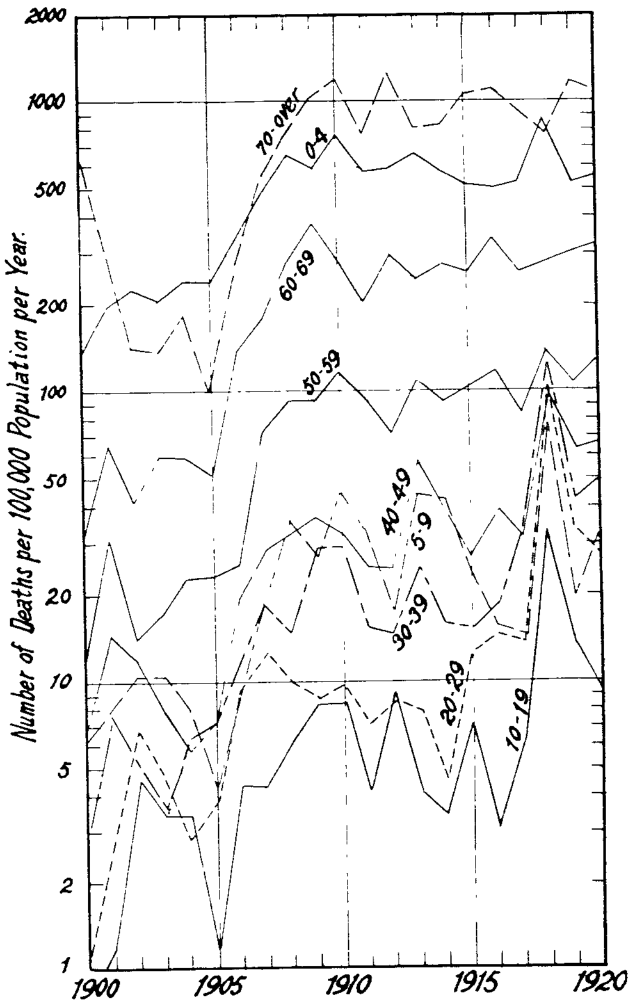

FIG. 5. Age specific death rate from Broncho-pneumonia in Pittsburgh, Pa., during the period 1900–1920.

will differ—*e.g.*, the crude death rate from pneumonia, all forms, in Pittsburgh in 1920 is given in "U. S. Mortality Rates, 1910–1920," as 342.8 on pp. 19, 110, 586 and as 341.3 on pp. 108, 287.

III. Discussion of the Trend.

8. From the figures for the age specific death rates per hundred thousand the yearly trend for the period 1900–1920 has been com-

TABLE II. (*continued*).

B. Bronchopneumonia. (Fig. 5.)

Age Group.	1900.	1901.	1902.	1903.	1904.	1905.	1906.
0–5	131.6	198.0	224.0	204.0	238.4	234.0	330.0
5–9	6.3	8.4	10.4	10.4	8.3	4.1	8.3
10–19	0	1.2	4.6	3.4	3.4	1.1	4.4
20–29	1.0	2.8	6.9	4.8	2.8	3.8	9.2
30–39	2.7	7.7	5.1	3.7	6.1	7.2	11.7
40–49	6.4	14.4	12.0	7.7	5.7	7.4	19.8
50–59	11.0	32.1	13.8	16.9	23.0	22.4	25.0
60–69	29.8	65.2	41.9	61.0	59.1	51.3	137.0
70 on	66.2	323.0	141.5	138.0	180.0	102.5	286.0
	1907.	1908.	1909.	1910.	1911.	1912.	1913.
0–5	485.0	649.0	586.0	797.0	575.0	590.0	663.0
5–9	18.6	35.0	26.7	45.0	32.2	17.8	44.7
10–19	4.3	6.4	8.3	8.2	4.1	9.2	4.1
20–29	12.6	10.0	8.9	9.7	7.0	8.8	7.9
30–39	18.6	14.7	28.9	29.6	16.3	15.0	25.5
40–49	28.0	32.5	36.8	32.6	25.6	25.0	59.7
50–59	73.4	92.5	93.0	114.0	99.4	72.2	109.2
60–69	176.0	282.0	372.0	290.0	204.0	299.0	249.2
70 on	533.0	755.0	1030.0	1190.0	757.0	1260.0	704.0
	1914.	1915.	1916.	1917.	1918.	1919.	1920.
0–5	572.5	504.0	500.0	520.0	888.0	520.0	556.0
5–9	42.0	22.6	16.6	14.5	80.5	19.3	32.9
10–19	3.4	7.1	3.0	6.0	34.0	13.9	9.0
20–29	4.5	12.5	14.3	13.4	98.2	34.2	25.9
30–39	15.7	15.6	18.7	34.8	126.0	43.2	49.7
40–49	39.0	26.5	39.0	32.6	103.0	64.0	65.7
50–59	91.0	102.0	114.0	85.9	138.0	108.0	127.0
60–69	277.0	200.0	334.0	269.0	280.0	303.0	313.0
70 on	816.0	1060.0	1080.0	922.0	760.0	1156.0	1096.0

puted (Table III.), in most cases both with the abnormal influenza year and without it. The trends with 1918 excluded are probably more significant. It is noticeable that bronchopneumonia and pneumonia, all forms,[9] are increasing at all ages and that lobar and undefined pneumonia is increasing in all age groups other than infancy and 0–4. It should be emphasized that these figures have naturally to do only with the reported statistics and not necessarily with actual pneumonia. As the rates in the different age groups differ so much it is illuminating to compare the rate of yearly increase with the mean.

[9] Compare with Fig. 4 which shows that for the ten original registrative states the pneumonia mortality is sensibly stationary in all age groups.

TABLE II. (*continued*).

C. Lobar and Undefined Pneumonia. (Fig. 6.)

Age Group.	1900.	1901.	1902.	1903.	1904.	1905.	1906.
0–5	518.0	459.0	593.0	554.0	459.0	501.0	344.0
5–9	21.0	46.0	48.0	41.5	35.4	33.2	35.2
10–19	27.4	37.5	38.0	33.0	27.7	24.3	32.6
20–29	63.5	51.0	83.0	76.0	65.4	68.2	59.0
30–39	80.7	76.5	108.0	105.0	83.2	114.0	100.0
40–49	108.0	150.0	216.0	159.0	125.0	138.0	137.0
50–59	220.0	222.0	243.0	229.0	210.0	235.0	190.0
60–69	470.0	377.0	566.0	536.0	368.0	460.0	322.0
70 on	927.0	985.0	950.0	720.0	1156.0	1055.0	673.0
	1907.	1908.	1909.	1910.	1911.	1912.	1913.
0–5	210.0	234.0	138.0	259.0	132.0	193.0	192.0
5–9	22.6	39.0	20.6	20.4	10.1	17.8	31.0
10–19	28.9	32.8	35.5	29.8	15.7	27.6	26.4
20–29	61.6	57.7	78.4	96.6	54.0	67.5	92.0
30–39	69.2	89.5	121.6	168.0	111.0	120.0	158.0
40–49	92.0	200.0	199.0	248.0	140.0	232.0	220.0
50–59	194.0	235.0	356.0	428.0	254.0	340.0	364.0
60–69	344.0	545.0	585.0	752.0	410.0	703.0	648.0
70 on	742.0	932.0	1060.0	1390.0	1020.0	1240.0	1370.0
	1914.	1915.	1916.	1917.	1918.	1919.	1920.
0–5	165.0	189.0	211.0	294.0	641.0	274.0	243.0
5–9	24.8	16.9	40.6	27.2	206.0	44.0	38.0
10–19	29.4	32.4	34.2	48.1	274.0	57.0	50.5
20–29	50.0	60.0	116.0	147.0	835.0	160.0	139.0
30–39	119.0	116.0	202.0	275.0	1030.0	220.0	213.0
40–49	180.0	200.0	375.0	378.0	787.0	263.0	268.0
50–59	356.0	368.0	516.0	597.0	767.0	344.0	362.0
60–69	523.0	612.0	864.0	680.0	1040.0	505.0	665.0
70 on	1190.0	1072.0	1500.0	1545.0	1430.0	970.0	1300.0

This table shows that the rate of increase of the pneumonia mortality is greatest in just those ages 20–50 which are economically the most important with respect to labor and that the rate of increase[10] averages over 5.5 per cent. per annum for the whole period 20–60.

[10] With reference to the calculation it should be observed that although the rates are given on the graphs on a logarithmic scale and that trend lines on such graphs represent a geometrical law of increase, the trends calculated have been computed from the rates themselves and not from their logarithms and are thus above the true geometric rates. A geometric rate of 1.05 per annum, as may be seen from interest tables, carries $1.00 to $2.65 after 20 periods and corresponds to a mean arithmetic rate of 8 per cent. based on the initial principal but to about 5 per cent. based on the mean principal. Conversely an arithmetic rate of 5.5 per cent. per annum, from 1900 to 1920 carries 100 in 1900 to 210 in 1920, a result which would be accomplished by a geometric rate of 3.8 per cent.

TABLE II. (*continued*).

D. Pneumonia in Infancy. (Fig. 7.)

Year.	Bronchopneumonia.	Lobar or Undefined Pneumonia.	Pneumonia All Forms.
1900	279	1160	1439
1901	529	1220	1749
1902	627	1460	2087
1903	582	1450	2032
1904	700	1300	2000
1905	585	1170	1755
1906	842	830	1672
1907	1300	503	1803
1908	1490	525	2015
1909	1590	321	1911
1910	2020	494	2514
1911	1550	296	1846
1912	1510	572	2032
1913	1630	378	2008
1914	1800	437	2237
1915	1610	378	2188
1916	1430	572	2002
1917	1750	722	2472
1918	2090	1260	3350
1919	1710	697	2407
1920	1715	615	2330
Mean	1301.8	779.04	2088.0
Yearly increase	+77.73	decrease −35.68	increase +42.05

TABLE III.

YEARLY TREND OF PNEUMONIA DEATH RATE IN DIFFERENT AGE GROUPS, 1900–1920.

Age Group.	Bronchopneumonia.		Lobar and Undefined.		Pneumonia, All Forms.	
0–4	+24.6	+21.8[7]	−12.7	−16.4[7]	+11.9	+ 5.4[7]
5–9	+ 1.7	+ 1.1	+ 1.6	− 0.2	+ 3.3	+ 0.9
10–19	+ 0.5	+ 0.3	+ 3.1	+ 0.8	+ 3.6	+ 1.3
20–29	+ 1.8	+ 1.7	+11.1	+ 4.0	+12.9	+ 5.7
30–39	+ 2.7	+ 1.8	+15.9	+ 7.8	+18.6	+ 9.6
40–49	+ 3.1	+ 3.7	+14.4	+ 9.5	+17.5	+13.2
50–59	+ 5.9	[8]	+16.0	+12.9	+21.9	+18.8
60–69	+12.7	[8]	+16.1	+11.5	+28.8	+27.6
70 up	+54.2	+57.3	+25.8	[8]	+80.0	+83.1
0–1	+77.7		−35.7		+42.0	

[7] Values of the yearly trend, when the influence of the year 1918 (influenza epidemic) has been eliminated (instead of the real numerical values of the specific death rate in 1918 the mean specific death rate of the years 1917 and 1919 has been taken).

[8] Not affected by the year 1918.

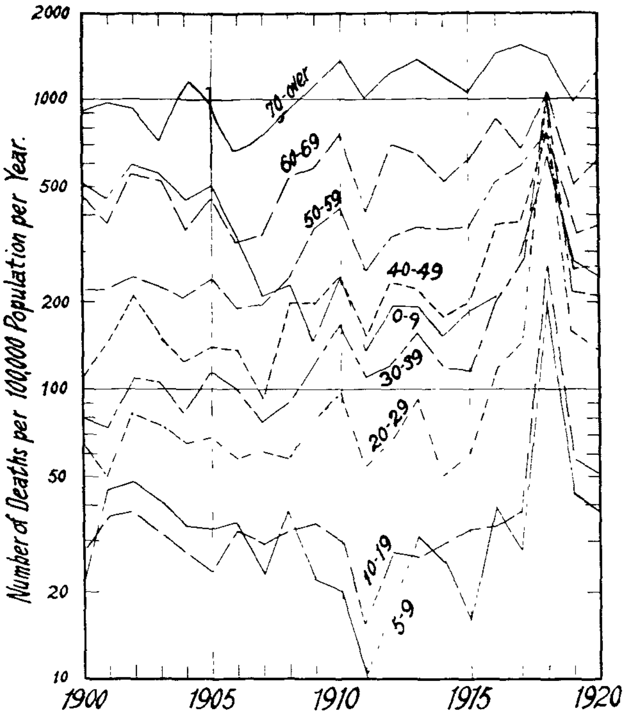

FIG. 6. Age specific death rate from Lobar and Undefined Penumonia in Pittsburgh, Pa, during the period 1900–1920.

TABLE IV.

YEARLY INCREASE COMPARED WITH THE MEAN, 1900–1920.

Age Group.	Mean.	Trend.	Trend : Mean.
0–4	768	5.4	0.7%
5–9	52	0.9	1.8%
10–19	40	1.3	3.3%
20–29	96	5.7	5.9%
30–39	158	9.6	6.0%
40–49	237	13.2	5.6%
50–59	395	18.8	4.8%
60–69	813	27.6	3.4%
70 up	1803	83.1	4.6%

Although the average trend has been calculated for the 21 years with adjustment for 1918 the curves do not show a close adherence to this trend. If we take the first third of the period, 1900–1906, and judge by the eye, we should infer that the death rate from pneumonia

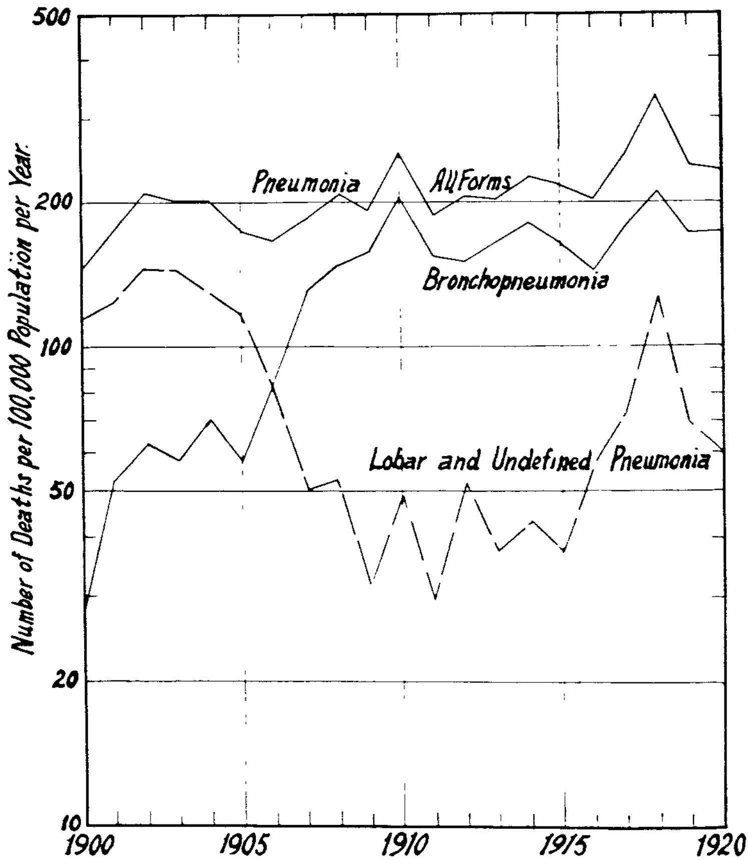

FIG. 7 Specific death rate from Pneumonia in Infancy in Pittsburgh, Pa., during the period 1900–1920.

all forms for these seven years tended to decline in most of the age groups. A calculation shows that the trends are

0–4	5–9	10–19	20–29	30–39	40–49	50–59	60–69	70 up
+4	0	−.4	+ .9	+4.8	0	0	−6	−6
+0.5%	0	−1%	+1.3%	+4.7%	0	0	−1.2%	−0.5%

In view of the short period, the trends are not well established statistically but the only percentages that approach those for the whole period are for the age groups 0–4 and 30–39; in all other age groups except 20–29 the trend is zero or negative. The disturbance of the influenza epidemic makes it difficult to treat separately the last part

of the period; but a comparison of the means for 1900–1906 (central year 1903) and for 1910–1916 (central year 1913) is interesting.

	0–4	5–9	10–19	20–29	30–39	40–49	50–59	60–69	70 up
1903	713	45	34	71	102	158	243	506	1100
1913	791	55	34	86	162	263	475	909	2236
Inc.	78	10	0	15	60	105	232	403	1136
Mean	752	50	34	78	132	210	359	707	1668
Trend	1.0	2.0	0	5.2	4.5	5.0	6.4	5.7	6.8
Cf.	0.7	1.8	3.3	5.9	6.0	5.6	4.8	3.4	4.6

In the age group 70–up the increase was rapid between 1906 and 1910, before and after which the rate was nearly constant. In the groups 60–69, 50–59, and 0–4 much the same holds true. In all four of these groups the trend estimated from the two seven-year periods 1900–1906 and 1910–1916 was markedly larger than when calculated for the whole period 1900–1920. In the age groups 5–9, 20–29, 40–49 the roughly estimated and the calculated trends are practically the same. In the groups 10–19 and 30–39 the trend is largely due to the rapid increase of the death rate in the last few years. We have as yet been unable to decide upon any rational explanation of these differences between the course of the mortality between the different groups. Some of the differences may be accidental, some of them probably are real and significant. At any rate there has been for all groups a considerable average increase in mortality from pneumonia all forms with a particularly sharp increase in the old ages 50 and up between 1906 and 1910, acting as though the mortality for these ages were moving from a lower to a higher level.

9. A comparison between Boston and Pittsburgh is interesting—one a seaboard, the other a mountain valley city, both damp and cold, not dissimilar in size. The population of Boston is older, *i.e.*, the percentage of the population in the different age groups below 40 is less than in Pittsburgh and in the groups above 40 greater. The figures for pneumonia all forms 1910–1920 with means and trend, and the age specific death rates for 1920 are given:

It is noteworthy that the pneumonia mortality in Boston appears to be decreasing while that in Pittsburgh increases. The rates of trend are — 1.1 per cent. and + 3.3 per cent. relative to their means. The mortality rates are crude, but the effect is so pronounced with a

Pneumonia, All Forms, 1910–20.

Year.	Boston.	Pittsburgh.
1910	214	324
11 . ..	210	208
12.... .	206	263
13. .. .	200	287
14. .. .	196	244
15......	210	249
16.. .	220	337
17..	223	364
18. ..	192*	348*
19 . . .	162	331
1920 . ..	198	341
Mean. ..	203	300
Trend....	−2.2	+9.8

* Mean of 1917 and 1919.

Age Specific Rates for 1920.

Age.	Boston.	Pittsburgh.
0–1 .	2473	2338
1–4 . .	361	451
0–4	774	816
5–9 .	43	71
10–14 .	21	27
15–19.	32	96
20–24..	70	143
25–34. ...	90	208
35–44. .	115	292
45–54	154	386
55–64	203	737
65–74..	682	1457
75 up.. .	1550	2886

differential rate of 12 per hundred thousand per annum that no adjustment for population would materially change the divergence. The adjustment of the Boston and Pittsburgh figures for 1900 and for 1920 to the English standard million gives for Boston in 1910, 227 and 206 in 1920; for Pittsburgh in 1910, 365 and 359 in 1920. It is also noteworthy that in the age groups the rates for infants are the only ones which show Boston in the lead (the difference being barely significant statistically); in all other groups Pittsburgh leads and leads enormously in the older groups,—from age 15 on the rates for Pittsburgh are more than double Boston's until the final group which contains relatively few deaths.

IV. Suggested Reasons for Pittsburgh's High Rate.

10. Many and divers are the suggestions that have been made to explain the high pneumonia rate in Pittsburgh. (1) The differential mortality of race stocks may contribute. Dr. L. I. Dublin, analyzes in *Quart. Publ. Amer. Statist. Assoc.*, March, 1920, the mortality of race stocks in Pennsylvania and New York based on data for 1910. He finds for the 1910 death rate for persons over 10 years of age This result shows that the foreign born and the natives born of foreign or mixed parentage have decidedly higher adjusted death rates, say 40 per cent. higher. The crude rates show the same sort of increase in mortality in the case of foreign born, but owing to the more favorable age distribution among the natives of foreign or

	Crude Death Rate. Pennsylvania. M	F.	N. York M.	F.	Standardized Rate.* Pennsylvania. M.	F.	N. York. M.	F.
Native white, native parents	11.5	10.9	13.8	12.4	12.8	12.3	13.8	12.4
Native foreign or mixed parents	12.6	10.0	13.2	9.7	18.8	16.3	19.5	15.5
Foreign born white	16.5	16.9	17.5	16.6	17.5	16.0	17.3	16.2

* The standardization is made against the New York distribution of native white born of native parents, 1910 figures, over 10 years of age.

mixed parentage the standardization is necessary to bring out the increase in their case.

The percentage of foreign born in 1910 and 1920 respectively for Boston was 36.3 and 32.4; for Pittsburgh 26.4 and 20.5. If therefore the foreign born, not further subdivided, were to be an explanation of the high pneumonia rate, the effect should be more noticeable in Boston than in Pittsburgh. If the foreign born had a rate 40 per cent. higher than the natives of native parentage an admixture of 1/3 or 1/4 or 1/5 of them would increase the general rate only 13 per cent. or 10 per cent. or 8 per cent. respectively. Dr. Dublin also gives the rates in some age groupings for some nationalities for pneumonia all forms and for certain other diseases.

PENNSYLVANIA PNEUMONIA RATES 1910, MALES, AFTER DUBLIN.

	Native White.	Pitts-burgh.*	Boston.*	Foreign Born. Austria Hungary.	Russia.	Germany.	Ireland.	Italy.
Und. 10	412.1	450	420	193.3	138.8	74.0	122.4	198.8
10–14	14.9	27	21	....	16.6	....	....	92.2
15–19	27.9	96	32	61.6	49.7	45.4	....	130.2
20–24	26.6	143	70	53.3	27.9	42.3	59.3	110.1
25–44	67.0	250	100	70.6	67 3	112.5	94.7	92.8
45–64	166.0	490	175	260.4	237.2	269.1	343.5	348.8
65–84	498.2	1700	900	485.4	520.4	917.2	758.9	722.0
All	175.5	341	198	90.9	84.2	293.4	530	135.4

* Pittsburgh, 1920, estimated for this age distribution; the figures for 1910 were higher. Boston, 1920, estimated; the figures for 1910 were higher.

Now in Boston the Irish make about 30 per cent. of the foreign born, the Russians and Italians about 15 per cent. each, Austria-Hungarians and Germans only about 5 per cent. together. In Pittsburgh the Irish are about 15 per cent. of the foreign born, the Germans and Austria-Hungarians about 20 per cent. each, the Russians and Italians about 10 per cent. each. Recalling that the foreign born are nearly

half again as numerous relatively in Boston as in Pittsburgh, it is difficult to see how Dr. Dublin's figures can help us toward the conclusion that the high rate in Pittsburgh can be due in any large measure (as compared with Boston, for example) to the extra mortality of pneumonia among the foreign born; there is probably an appreciable effect of this influence in raising the pneumonia mortality in both cities, but how great can hardly be determined until better figures are available.[11]

The situation with respect to negro mortality from pneumonia is interesting. For the 11 years 1910–1920 inclusive the rate in Boston for negroes averaged 340 as against 200 for whites and there was an increase of 3.0 per annum for the former as against a decrease of 2.5 per annum for the latter. The total effect upon the figures for Boston was not great because the negro population was only about 2.2 per cent. during the whole time. In Pittsburgh the rate for whites during this period was 285 and for negroes 510. The increase for whites was 8 per annum and for negroes was 33 per annum. This makes a considerable difference because the negro population was larger than in Boston and was in fact increasing from about 5 per cent. in 1910 to 6½ per cent. in 1920. During the same decade while the total pneumonia rate in Chicago was dropping sharply the rate for negroes was rising slightly. It appears therefore that in Boston and Chicago during this period the trend of the pneumonia death rate for negroes was opposite that for whites and that in Pittsburgh although the rate for whites rose, that for negroes rose very much faster. It appears, therefore, that of the total rise in the Pittsburgh rate from 1910–1920 about 20 points was due to the high and rapidly increasing mortality rate from pneumonia for negroes. This represents about 20 per cent. of the total increase. We are at a loss to explain these figures. Several suggestions could be made and we feel confident that a critical analysis of the situation relative to the negro mortality might go a long way toward explaining Pittsburgh's anomalous situation which respect to total pneumonia mortality. Of the excess of Pittsburgh's average rate over Boston about

[11] We must bear in mind that we have to explain the increase in Pittsburgh from normal figures in 1900 to abnormal figures in 1920; the changes in population in this period have not been such as to make any marked effect.

10 per cent. is attributable to the difference in percentage of negro population.

11. (2) The sex distribution in Pittsburgh is favorable to a high mortality rate from pneumonia owing to the greater mortality in males than females. We have not analyzed the Pittsburgh figures by sex any more than we have by race, because we had no desire to overburden with requests for statistical material the very obliging Health Department of the City of Pittsburgh; a simple hypothetical calculation will show the order a magnitude of the effect due to sex distribution. Let r be the male rate, s the female rate, p and q the fractions of the population which are male and female respectively. Then the total rate is $R = pr + qs$. If we change p to $p + x$ and q to $q - x$ the rate R changes to $R + x(r - s)$. Now it is generally believed that the rate for males runs about 25 per cent. higher than for females. For example $r = 200$, $s = 160$ and $R = 180$ if $p = q$ are fair comparative figures. In general $r - s$ is some fraction not far from 1/4 of R. A shift from 48 per cent. males to 52 per cent. males in the distribution of population makes $x = .04$ and changes R by 1 per cent. Such changes are entirely masked by so many other variables that it would take an elaborate study to detect them. It is fair to conclude that unless the mortality in pneumonia in Pittsburgh is different in the two sexes to a degree far greater than usual, and in fact to an unbelievable degree, the sex distribution of itself would not materially affect or explain the excess. Taking race and sex distribution together it would be difficult to find a 20 per cent. disadvantage in Pittsburgh as compared with Boston whereas the mean difference 1910–1920 was 50 per cent. and the trends were opposite and were producing a divergence at the rate of between 4 and 5 per cent. per annum, while the progressive changes in sex and race distribution in the two cities showed no marked differences in trend.

The U. S. Mortality Rates 1910–1920 gives the sex rates in age groups for pneumonia, all forms. A few figures may illuminate this discussion.

It is seen that the sex ratio M. F. for pneumonia mortality in Pittsburgh in 1920 exceeded that in Philadelphia (and it is true that it exceeded normal) but was not markedly higher. The number of cases in the groups are small but the ratios in the hard working ages

	Pittsburgh 1910. M.	F.	M. F.	Pittsburgh 1920. M.	F.	M. F.	Philadelphia 1920. M.	F.	M. F
0–1	2960	2040	1.45	2645	2027	1.30	2214	1713	1 29
1–4	694	651	1.07	533	367	1.46	392	302	1.30
0–4	1191	950	1.25	946	687	1.38	754	584	1.29
5–9	85	46	1.8	82	60	1.4	48	44	1.1
10–14	30	13	2.3	38	15	2 5	32	21	1 5
15–19	85	23	3.6	117	76	1.5	42	60	0.7
20–24	121	49	2.5	172	117	1.5	94	79	1.2
25–34	211	93	2.3	245	170	1.4	136	134	1.0
35–44	302	122	2.5	348	231	1.5	147	118	1.2
45–54	494	196	2.5	426	339	1.3	169	131	1.3
55–64	1048	569	1.8	936	537	1.7	318	219	1.5
65–74	1762	1285	1.4	1651	1291	1.3	620	557	1.1
75 up	3078	3346	0.9	2770	2967	0.9	1009	1082	0.9
Total.	391	255	1.53	393	292	1.35	208	163	1.25

tend to exceed those for Philadelphia more than in other age groups. In 1910 the ratios M./F. are prodigious and from 20 to 55 are markedly higher than for under 10 or over 55. (The 10–14 and 15–19 group contains very few cases.) Whatever the effect of sex distribution, it should show a decrease from 1910 to 1920. It is thus probably not the excess liability of males to death from pneumonia *per se* that is effective.

12. (3) The long hours of heavy labor and exposure to high temperatures have been suggested as causes of high mortality from pneumonia in steel workers. Such working conditions are unpleasant and make an unpleasant effect upon social welfare surveyors. One need not fail of sympathy with the worker while pointing out that apparently what look like almost inhuman conditions in industry seem often to appeal to our immigrant labor as less undesirable than going upon our farms and working the land as they did in their own countries. At any rate the question to be determined is whether the industrial conditions do or do not materially increase the pneumonia death rate, and to what extent. This would require the careful assembling of statistical material not now at hand. The situation is complicated by the fact that when the industry is active there is both overwork, large additions to the working force, and steady wages even if not at a particularly high level, whereas when the industry is dull there is idleness and often not enough earnings to support existence. Overwork with pay versus failure to get work or pay make

a situation which in the net might make little difference with the pneumonia death rate or might influence it relatively in either direction. If the industry in Pittsburgh would as a whole for an experimental period of one decade take an interest in the health of its workers comparable to that taken by the Edison Illuminating Company of Boston, The Hood Rubber Company of Watertown (Mass.) and a number of scattered industrial concerns throughout the country, we might be in a position to make some well-founded statements concerning the influence of industrial activity on disease and death. Incidentally we believe that such attentions to the health of the worker would pay the industry in dollars and cents. But the investigation might not be so fruitful in scientific result or in increased profit in the case of so irregular an industry as steel as it should be in a public utility with a reasonably steady load.

So far as the secular increase of pneumonia is concerned we should have to assume that the industrial conditions were becoming during the period 1900–1920 increasingly severe [12] or that a larger proportion of the population were becoming involved in the long hours of heavy labor with exposure to high temperatures. Furthermore such an acceleration would have to apply also in the age groups 70–up, 60–69, 50–59 which are mainly too advanced to be largely made up of persons engaged in that sort of work. The effect should be particularly marked in the hard labor ages 20–29, 30–39, and these groups were so largely affected by the influenza epidemic that inferences from their course of mortality is difficult. There is a touch of suggestion in the figures for the war years 1915–16–17 before the epidemic that these two groups (and the group 40–49 to a less extent) are being adversely affected as compared with other groups; for the rate of increase of pneumonia mortality in these groups was much greater, and even greater than the rapid rise of 1906–1910 in the groups 50–59 and 70–up. It is merely a suggestion, but could be interpreted as a reflection in the working ages of the forced ac-

[12] It was during this period that in many places the Taylor and other systems of estimating efficiency of laborers with their tendency to stimulate speeding up were progressively coming into some vogue; we have been unable to obtain the data that would let us even so much as imply that such methods were responsible for any part of the Pittsburgh acceleration of pneumonia mortality—if acceleration there, why not elsewhere?

tivity due to the war. Another straw blowing down the same wind is the larger excess of male as compared with female mortality during these ages in Pittsburgh. (See above.) One real difficulty of correlating industrial activity and pneumonia mortality lies in the fact that pneumonia and the steel industry do not respect the division of time into calendar years. Pneumonia is a winter disease and its rise and fall is cut in two by the date January 1 without being in any sense cut in halves. The peak of pneumonia comes in different months in different winters; it is not unusual to have two peaks in the same years or to find years with no peaks; we should have a better picture of the yearly situation relative to pneumonia if the statistical year were July 1–June 30. The general industrial cycle based on the figures of the past 40 years seems to run its course in about 40 months. Properly to discuss the effect of industry on pneumonia mortality we need monthly figures.

13. (4) The labor turn-over may well be an item that should be influential in the increase of pneumonia. Insofar as this turn-over represents mere restlessness of an urban population of wage-earners going from one position to another in the same city or metropolitan district, the effect would be nil. But if to keep places filled there is any great importation of labor from rural districts, of persons previously less exposed to city diseases and thus possessing less of acquired immunity, there should be expected in this new population an excess of such diseases of which pneumonia appears to be one. If a person coming to Pittsburgh contracts pneumonia and dies of it, his death figures in our statistics. To a certain extent this sort of non-resident or temporarily resident death influences the mortality statistics of most large cities. The question relative to Pittsburgh is: How great is the influence? It is rumored that when industry is active train load after train load of laborers, foreign, rural, and negro, are brought to Pittsburgh, many of them departing after a short stay. Rumor does not afford a good basis for statistical inference, despite the many inferences based on little else.

As we desired to have figures we applied to the Pittsburgh Chamber of Commerce and to various individuals connected with the industry, with no definite result. A foreman estimated that he took on 100 new hands per year to keep 100 places filled, in a mining dis-

trict a superintendent said he hired 5,000 during the year to maintain a force of 1,000, a large operator had to take on 2,000 persons each year for each 1,000 permanent positions. Such figures would not be extreme if one may take as standards the turn-over rate we have found in some industries certainly no more variable than the steel industry and offering work and working conditions fully as attractive. The figures are not extreme compared with those of Brissenden.[13] The cost of turn-over is variable, but high. The lowest average estimate for a considerable plant that has been given us is $8 per change and in the same plant another official gave the rate as $150 per position. So many considerations enter into the proper cost accounting of turn-over that estimates are liable to be wider of the mark for the cost than for the turn-over itself. It would be interesting to have real figures and it is not impossible that they would show results not alone of scientific value relative to the health of the industrial worker but of cash value to the industry. We are inclined to believe that labor mobility, particularly the immigration of foreign, rural, and negro labor with the Pittsburgh district is responsible for some of the excess pneumonia mortality there; but we lack any evidence that relatively speaking, the mobility has been increasing rapidly during the period in question.

14. (5) The housing conditions described on the basis of the survey of 1907–1908[14] as inimical to the public health have been greatly improved. There are still, to be sure, over-crowding, inadequate sanitation, cellar dwellings, insufficient water, foul privies, unventilated sleeping rooms, disorder, and dirt, and one may yet find rooms sleeping 6, 8, or 10 lodgers and occupied by two shifts of sleepers. Such conditions with close personal contact inevitably involve great liability to the rapid spread of pneumococcus infection; but both the Department of Health and the Industry in Pittsburgh have been steadily working to ameliorate the conditions which may now be considered comparatively satisfactory. Not much help in analyzing the situation can be obtained from general statistics. Both in 1910 and 1920 Boston is reported as having 50 per cent. more per-

[13] *Jour. Amer. Statist. Assoc.*, December, 1923.

[14] The Pittsburgh District, pp. 87–138, "The Housing of Pittsburgh's Workers," Russell Sage Foundation, New York, 1914.

sons (9.1, 9.4) to a dwelling than Pittsburgh (6.1, 6.3) and both cities show an increase of 3 per cent. for the decade in the number to a dwelling although with a decrease of 6 per cent. in the number of persons (4.8, 4.5) to a family which is at both times the same in Boston and Pittsburgh. It is probable that the acute housing shortage which came with the cessation of building during the war is responsible for these changes; it is not unlikely that with the élan of activity in all lines during the war better housing conditions were like many other improvements disregarded. Yet we are not concerned with explaining merely the high rate of pneumonia mortality in Pittsburgh but the increasing rate as compared with other cities. Housing conditions so far as we have been able to survey them will not account for the large differential trend against Pittsburgh.

15. (6) Smoke and dust are a favored pair on which to blame the pneumonia mortality in Pittsburgh. True, some have suggested that smokes contain phenol, chlorine, and possibly other mild disinfectants for the respiratory tract; some have tentatively surmised that carbonaceous particles were a palliative for the irritation set up by silicious dust. But it is difficult to believe that smoke and dust regularly inhaled are of a net advantage to the lungs any more than they are to a delicate mechanism like a watch; the cloying and fouling action must outweigh any incidental cleansing effects. The Mellon Institute conducted a smoke investigation and published in 1914, Bulletin No. 9, *Papers on the Influence of Smoke on Health.* The case against smoke was pretty thoroughly established. A chart (p. 158) shows the average pneumonia death rate in 1910 and 1911 and the average soot fall per month in each of the 27 wards of the city. The correlation is very close; a computation of the coefficient of correlation from values obtained by reading the chart gives $r = 0.92$. This is not conclusive; there is a good deal of correlation between the soot fall and the death rate from tuberculosis and between the soot fall and the number of persons per acre (taken as an index of poverty). It is possible that the greater part of the correlation is due to some underlying variable which forces the cogredient variation of soot fall and pneumonia mortality. Even if smoke were not physically harmful it is undesirable esthetically and there must be a strong tendency for the persons best able to provide for

themselves to shun the smokiest districts, leaving them to those not only poorer but less concerned about hygienic conditions. To treat the problem statistically long continued and carefully supervised collections of data would be necessary. In the final analysis one should have to come to partial correlation.

There has been a great deal of agitation in Pittsburgh against the smoke nuisance, which may probably be called also the smoke menace. Is it possible that conditions are getting worse rather than better? Opinions that we have obtained differ. Observations show that today the inhabitants of Pittsburgh are much troubled with foreign particles in the eyes—a condition which is even written up in a semi-humorous fashion in the press. Some say conditions are worse, others that they are better. We have tried to get actual data on the amount of precipitation of soot and ash, together and individually, over a period of years, but without success. Such figures ought to be kept and to be available.[15] If an excess of pneumonia mortality is due to smoke it is not necessary that the smokiness worsen throughout a period of rising mortality; the deleterious effects of the accumulation of foreign matter in the lungs may be cumulative and the mortality at any age may depend on the average length of time the persons of that age have been exposed to smoke.[16] That there may be such cumulative action is supported by the findings at autopsy by Dr. S. R. Haythorne [17] not only of the severe anthracosis but of the tendency of that anthracosis to prevent or delay the resolution of exudates in pneumonia. The suggestion is clear that in the case of anthracosed lungs the pneumonia may lead more frequently to fatality than in normal lungs and that part of the excess mortality in Pitts-

[15] Smokiness as reported by the weather observers might or might not be of value as an index in a qualitative way of probable conditions in the inhaled air; but quantitative measures are needed. In an article entitled Pneumonia from a Public Health Point of View, *Pittsburgh Medical Bulletin*, Vol. 13, No. 42, December 13, 1924, Dr. C. J. Vaux reports findings by the Mellon Institute, August, 1923 to June, 1924, showing that the precipitation of insoluble matter from the air is about 40 per cent. greater than during the same months eleven years earlier.

[16] In this case an increasing population in the several age-groups might bring either a stationary, a rising, or a falling pneumonia mortality according to the law of increase—a condition very difficult to analyze correctly.

[17] *Intern. Assoc. Med. Mus.*, No. 7, May, 1918.

burgh is excess fatality. Morbidity statistics are not reliable enough to justify a conclusion.[18]

16. We have tried to discuss briefly the suggestions that have been put forward to account for the excess and the increasing excess of mortality from pneumonia in Pittsburgh. It may be worth while to pause for a moment and count the cost of this excess. It is fair to estimate that in Pittsburgh there are today perhaps 150 and almost certainly 100 deaths per hundred thousand population per annum over and above what might be considered a reasonable rate (Boston, Chicago, and Philadelphia are running under 200 and Pittsburgh over 300). A typhoid fever death rate of over 25 is considered as indicating a negligent water or milk supply requiring a strenuous effort to improve the sanitary conditions, and Pittsburgh's typhoid rate which up to 1907–1908 had run well above 100 is now down around 10. The loss of 100 to 150 extra lives through pneumonia is not so striking as the same loss by typhoid (because the loss by pneumonia is inevitably large while that by typhoid should be small), but it is the same number of lives—some 600 to 1,000 each year in Pittsburgh. Considering the many factors involved it is not just to attribute all the excess to smoke, but the best evidence that we have, which is none too good, is that a considerable fraction of the excess is due to this cause. Man has always the problem of so improving his environment that he may live and what sanitarians have done for our water and milk they can if need be do for our air. The weakness of the situation lies precisely in the fact that we have not yet relative to the excess mortality from pneumonia anything like the hard and fast case against smoke and ash that we have against water in the case of typhoid. It is very important that the most extensive and critical investigations be carried on in regard to all aspects of the pneumonia situation in Pittsburgh before one jumps at conclusions which if taken seriously and acted upon with determination by sanitarians and public health officials would call for the expenditure of a great deal of money.

[18] In the case of pneumonia we should need the fatalities for the different types. If Pittsburgh were becoming a hot bed for the highly fatal Type III., much of the excess mortality might be thereby explained.

V. The Reliability of Pneumonia Data.

17. Among the items which must be analyzed is the reliability of the data for the pneumonia death rate. In the Mellon Institute Bulletin No. 9, above cited, White and Shuey (p. 164) have a paper in which they quote from a letter to them by Dr. John S. Fulton. One extract is worth reproducing as an introduction to our subsequent observations: "I would expect Chicago to have a higher pneumonia rate than Pittsburgh because there is a pneumonia obsession in the minds of the medical profession of Chicago." May there now be some stampede toward pneumonia in Pittsburgh? The disease is hard to diagnose, it occurs as a complication in many diseases and is complicated by others. If one is in a city with a high pneumonia rate one may become somewhat inured to the condition, and

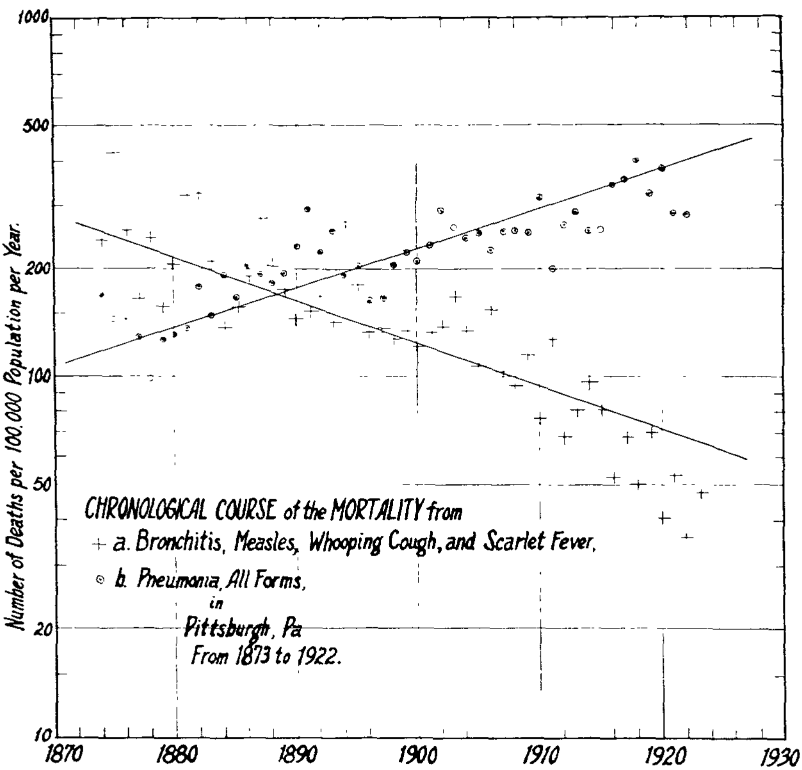

Fig 8.

report pneumonia in cases of doubt more frequently than is done in other places. Also as medical knowledge improves there are trends away from certain classifications and toward others. In the ten original registration states [19] chronic and acute bronchitis have practically disappeared in the period 1900–1920 and bronchopneumonia has much increased. The change is probably due rather to a shift in reporting than to a change in the incidence of disease. In 1910 lobar and undefined pneumonias were divided and the profession returned 50 per cent. undefined, 50 per cent. lobar, but by 1920, the eleventh year after the division, undefined pneumonia had practically disappeared whereas lobar pneumonia had increased enough nearly to make up for the loss; again a matter of shifting diagnosis. We

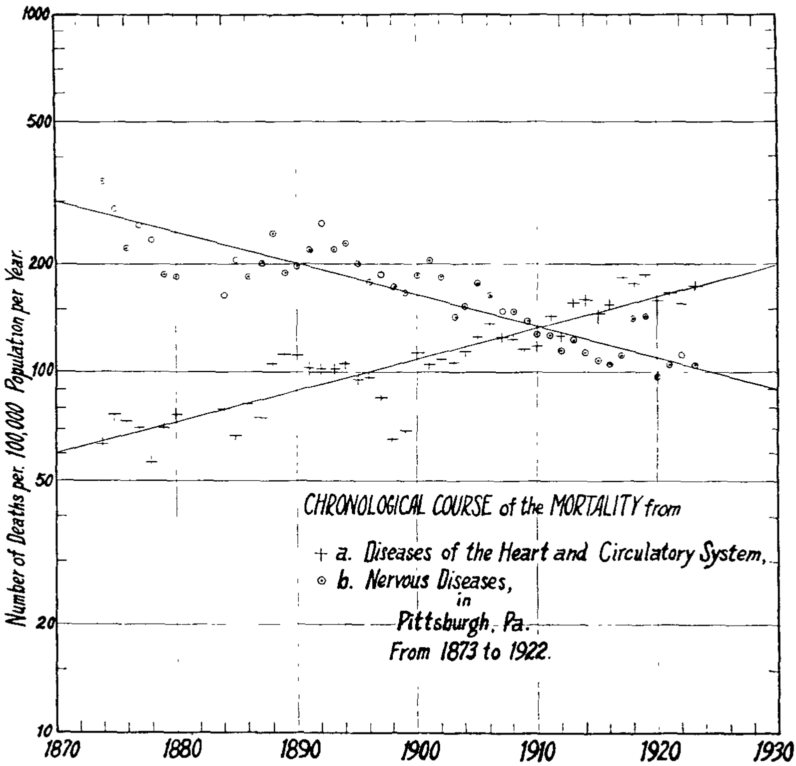

FIG. 9.

[19] See Fig. 2, and for details our article in *Proc. Nat. Acad. Sci., Washington, 10,* May, 1924, pp. 161–166.

have two long range figures for Pittsburgh and some comparisons between that city and Boston to offer for what light they may throw on this subject.

Figure 8 (Tables V., VI.) gives the chronological course of the crude mortality from (*a*) bronchitis, measles, whooping cough, and scarlet fever on the one hand and (*b*) pneumonia all forms on the other, for Pittsburgh 1873 to 1922, a full 50 years. It is noticeable that group (*a*) starting around 300 has practically disappeared being now below 50. Also group (*b*) starting around 125 has risen to about 325. Conditions for diseases have changed in Pittsburgh in a half century, the age distribution has altered; but with all allowances that can reasonably be made it seems clear to us that changes in reporting must enter. The next figure (9, Table 6) gives a similar presentation for (*a*) diseases of the heart and circulatory system increasing from 75 to 175, and of the rather ill-defined group (*b*) nervous diseases decreasing from 300 to 100. It is difficult to see how all of this can represent epidemiological changes of a biologic nature; some of it must be due to shift in classification.

18. Whereas Pittsburgh runs high in pneumonia, Boston is high in organic diseases of the heart. The following table represents the conditions at advanced ages (means of 1910 and 1920):

Age groups	Organic Diseases of the Heart. Boston.	Pittsburgh.	Pneumonia, All Forms. Boston.	Pittsburgh.	The Two Together. Boston.	Pittsburgh.
65–74	1758	1132	728	1579	2486	2713
75 up	4102	2433	1642	3007	5744	5440

It thus appears that if Pittsburgh is higher than Boston by 100 per cent. in pneumonia, all forms, in these age groups, Boston outdoes Pittsburgh by 60 per cent. in organic heart disease and that taking the two together in both age groups there is not much to choose. May it not be that Boston inclines to the one, Pittsburgh to the other, diagnosis, or is there a marked difference in reality as there is in reported causes in the two cities? Far be it from a Bostonian to imply that our diagnosis and reporting is better than that in Pittsburgh—particularly in view of the fact that for the five years 1916–1920 we have reported proportionately 2 1/2 times as many deaths as due to "unknown and ill-defined diseases" as have the Pittsburgh

TABLE V. (Fig. 8.)

BRONCHITIS, MEASLES, WHOOPING COUGH, SCARLET FEVER, SPECIFIC DEATH RATE PER 100,000 POPULATION, PITTSBURGH, 1873–1922.

Year.	Bronchitis.	Measles, Wh. Cough, Sc. Fever.	Total.	Year.	Bronchitis.	Measles, Wh. Cough, Sc. Fever.	Total
1873	50.7	189.9	240.6	8	49.2	85.0	134.2
4	90.5	338.0	428.5	9	57.8	64.9	122.7
5	59.3	198.6	257.9	1900	60.3	73.4	133.7
6	56.3	108.4	164.7	1	50.0	89.7	139.7
7	57.2	186.2	243.4	2	58.9	108.1	167.0
8	55.2	104.2	159.4	3	41.2	93.5	134.4
9	69.3	140.6	209.9	4	49.4	59.9	108.5
80	59.8	262.8	322.6	5	34.4	118.9	153.3
1	43.0	281.8	324.8	6	46.2	55.9	102.1
2	62.4	147.1	209.5	7	32.5	62.5	95.0
3	38.9	98.3	137.2	8	39.9	74.1	114.0
4	64.9	94.6	159.5	9	41.6	36.2	77.8
5	61.7	130.1	191.8	10	48.1	72.8	129.0
6	69.8	209.1	278.9	11	30.2	39.1	69.3
7	77.6	128.1	205.7	12	31.9	50.4	82.3
8	100.0	74.6	174.6	13	29.5	69.3	98.8
9	63.1	81.8	145.9	14	29.3	52.4	81.7
90	64.1	87.1	151.2	15	23.3	29.8	53.0
1	83.8	83.1	169.9	16	26.2	43.4	69.6
2	67.1	75.3	142.4	17	27.1	23.6	50.7
3	74.6	199.2	273.8	18	35.7	40.8	76.5
4	58.5	122.1	180.6	19	30.4	10.1	40.5
5	66.9	67.0	133.9	20	20.3	33.5	53.8
6	58.4	78.2	136.6	21	17.4	18.6	36.0
7	56.7	73.1	129.8	22	18.9	28.7	4.76

Trend for the total group = − 4.75 Trend for pneumonia all forms = + 4.81

profession! But the organic heart diseases in Boston run higher than Pittsburgh for the whole period 1910–1920, our average (crude rate) being 221 and theirs 131. There is less difference after adjustment to a standard population—the figures are then perhaps 223 and 156, but even so the rate for Boston is 43 per cent. or 67 per hundred thousand higher than Pittsburgh. So for the same decade excluding the influenza year Pittsburgh's pneumonia rate (crude) is 295 against Boston's 204 or after adjustment perhaps 322 against 214, a clear 50 per cent.

Other diseases manifest a similar persistent difference. Tuberculosis with us runs for 1910–1920 at about 136 (adjusted) but with all the smoke and dust Pittsburgh shows only 106. The disadvantage

TABLE VI. (Figs. 8, 9.)

DISEASES OF THE HEART AND CIRCULATION, NERVOUS DISEASES, PNEUMONIA ALL FORMS, SPECIFIC DEATH RATE PER 100,000 POPULATION IN PITTSBURGH, 1873–1922.

Year.	Diseases of Heart and Circulation.	Nervous Diseases.	Pneumonia, All Forms	Year.	Diseases of Heart and Circulation.	Nervous Diseases	Pneumonia, All Forms.
1873	63.2	346.6	148.1	1898	69.3	167.4	208.2
74	77.4	286.9	166.4	99	113.7	187.8	225.7
75	72.9	225.7	141.4	1900	104.5	203.3	210.3
76	70.4	259.9	140.8	1	108.1	182.5	233.4
77	51.7	233.8	126.9	2	107.2	143.6	295.4
78	70.3	189.3	99.3	3	113.6	160.2	264.1
79	76.0	186.0	125.3	4	125.1	198.2	243.5
80	***	***	129.8	5	135.8	162.7	251.3
81	***	***	133.9	6	124.1	149.0	228.3
82	***	***	180.0	7	122.2	146.8	257.1
83	79.4	164.0	149.1	8	116.8	139.1	256.7
84	67.0	208.6	193.0	9	119.2	127.0	253.8
85	81.5	186.6	167.4	10	142.7	127.5	318.7
86	74.6	208.3	203.6	11	126.6	115.7	199.8
87	104.8	246.2	194.3	12	156.7	123.3	261.1
88	113.2	190.9	183.6	13	159.2	113.6	282.5
89	110.0	198.7	194.3	14	145.6	108.3	251.5
90	102.7	218.3	233.0	15	150.8	106.9	252.6
91	102.4	260.7	299.6	16	181.4	110.1	243.7
92	102.0	220.0	222.4	17	174.0	141.2	257.7
93	104.5	229.5	257.6	18	184.0	143.9	669.6
94	94.5	200.7	193.4	19	157.3	98.5	321.9
95	96.7	178.9	202.2	20	164.1	105.7	380.5
96	85.7	185.8	163.2	21	152.8	112.5	287.1
97	65.7	173.9	167.7	22	173.7	104.1	284.9

of 30 per cent. is against Boston. Our rate for cancer and malignant tumors is also persistently higher, even after adjustment, than Pittsburgh's. It would be unreasonable to suppose that no such differences in the reported rates properly adjusted represented any real differences in the relative frequencies of the causes of death in different places; but it would be equally unreasonable, we believe, to hold that all such differences represent precisely the true relative state of affairs and it is for this reason that we have preferred to discuss in a general way some of the items which seem important in getting a fair notion of the situation regarding pneumonia in Pittsburgh. Progress is made by the study of exceptions as well as by investigation of the usual condition; the condition in Pittsburgh is so excep-

tional that such work as the Mellon Institute did should be continued intensively until a correct scientific understanding of the real causes of the exceptionally high pneumonia death rate in Pittsburgh has been obtained and may be made the sound basis for practical steps to ameliorate the situation.

19. The question will be asked: How do we estimate the apportionments of the various causes in making up for Pittsburgh a pneumonia rate 50 per cent. in excess of Boston's and increasing relative to it. The answer can be little better than guess work. We mentioned an estimate of not more than 20 per cent. as due to race (mostly negro) and sex distribution; this leaves 30 per cent. for other causes. The mortality of labor and the unhygienic housing conditions ought to affect not only the incidence of pneumonia but of other diseases. From the U. S. Mortality Rates, 1910–1920, we take the adjusted figures for Boston and Pittsburgh in 1910 and 1920 and average them.

No.	Disease.	Boston. Mean Rate.	Boston. % of Total.	Pittsburgh. Mean Rate.	Pittsburgh. % of Total.
1	Typhoid	6	0+	16	1−
2	Measles	14	1−	27	1+
3	Scarlet fever	11	1−	14	1−
4	Whooping cough	17	1	15	1−
5	Diphtheria and croup	26	2−	25	1+
6	Influenza	28	2−	58	3
7	Tuberculosis, lungs	129	8−	96	5+
8	Tuberculosis, other	29	2−	24	1+
9	Cancer, etc.	111	7−	89	5−
10	Diabetes	22	1+	15	1−
11	Cerebral Hem.	92	5+	88	5−
12	Organic heart Dis.	220	13	154	8−
13	Pneumonia all forms	216	13−	362	19+
14	Diarrhea-enteritis	94	6−	162	8+
15	Nephritis and Brights	74	4+	91	5−
16	Puerperal Septicemia	15	1−	29	2−
17	Puerperal other causes	23	1+	25	1+
18	Violent (not suicide)	84	5	124	7−
19	All other causes	489	29	504	27−
	All causes	1685	100	1884	100

Of these causes 1, 2, 3, 4, 5, 7, 8, 14, 16 seem to be those which should be influenced most by mortality of less immune population and close personal contact under unhygienic conditions (influenza is

omitted as possibly to be confused with pneumonia); the total contribution in Boston is 341 and in Pittsburgh 388, an excess of 11 per cent. Let us assume that a similar percentage would hold for pneumonia for the same causes. (The conclusion is of course largely guess work, particularly in view of the undistributed causes in No. 19.) Added to the previous 5 per cent., this makes 25 per cent.—though the question must clearly arise as to why conditions should be getting worse as compared with Boston.

As to the effect of hard conditions of labor the list of diseases gives little or no information. It seems impossible to venture any estimate, even so tentatively as in the other cases. In fact it might well be included in the previous figures; let us allow 5 per cent. and increase the 25 per cent. to 30 per cent. There remains 20 per cent. to be covered by smoke and dust on the one hand and systematic differences of diagnostic classification on the other. So far as we can judge these are the two causes that might most easily be cumulative or progressive in their effect, and we bear constantly in mind the necessity of explaining not only an excess but an increase of that excess. In the absence of any adequate statistical data we may blame the 10 per cent. on smoke and dust [20] and 10 per cent. on diagnosis.[21] We do not desire to stress these estimates, and in fact

[20] An intensification of industry should, on the smoke and dust theory, increase the pneumonia rate and even a continuation of living in a smoky atmosphere, provided more smoke is constantly inhaled than can be eliminated, should, as suggested, increase the rate at least up to a stationary condition. There are evidences that the rate is higher in old cities than in newer ones. An eminent health official who has worked in Cleveland remarked to us that now that city is nearly as smoky as Pittsburgh and ventured the suggestion that the pneumonia rate there should also be high. It is not; the mean for 1910–1920 (with adjustment for 1918) is only 143. But Cleveland has been increasing rapidly in its industrial development and as a matter of fact has been moving sharply against the general slow downward trend of pneumonia mortality in the original registration states. For the decade Cleveland's trend was + 7.8 (5.5 per cent.) and therefore comparable with Pittsburgh's. This suggested an examination of the figures for Detroit where the automobile industry has expanded so rapidly. The mean for the decade is 166 and the trend + 5.4 (3.3 per cent.). This sort of analysis is suggestive but unless very extensive and carefully controlled with production figures is hardly more than suggestive; a careful continued study of the physical, physiological, and epidemiological situation in one community might show a great deal more.

[21] Since the statement by Dr. Fulton about the pneumonia obsession in Chicago, above cited, was made (1912 ?), considerable time has elapsed. It is

give them mainly to stress the importance of properly analyzing the different factors and to emphasize our opinion that such an analysis on far more substantial basis than is apparently yet available is needed before the problem of Pittsburgh's pneumonia rate can be solved.

20. On April 1, 1924, pneumonia became notifiable and quarantinable in Pittsburgh. If notification is reasonably complete and if quaratine leads only to supervision of the cases by the City Health Department even without effective isolation we should in due time be in possession of valuable statistical material for a continuation of this study.[22] There may, however, come in a great complication in the matter of diagnosis. Quarantine, even the mildest, is not welcome and the inclination in cases of doubt, which are many, may be to avoid the decision of pneumonia when some non-reportable, non-quarantinable disease may equally well serve. This is suggested in no spirit of criticism; there need be no conscious effort to hide pneumonia and no sudden change, but an unconscious trend more or less

interesting to observe that the mean pneumonia death rate in Chicago for the decade 1910–1920 was 183, but with the remarkably sharp downward trend of − 7.6 (4.2 per cent.). The adjusted rate for 1920 was only slightly above (5 per cent.) the crude rate, whereas for 1910 the adjusted rate was much higher (16 per cent.). The trend was calculated from the crude rates (U. S. Mortality Rates, 1910–1920, p. 267) and would probably have been larger if calculated from adjusted rates. What has produced this sharp decline? Is the medical profession in Chicago recovering from an obsession or did the pneumonia death rate really drop from around 220 to around 150, and if so, why? [*Added in Proof.* As bearing on the possibility of over-crediting pneumonia with deaths, we may mention that in another industrial community with a very high pneumonia rate as reported there has been and still continuing an investigation by some of our clinical and experimental staff who are apparently finding that there has been in that community a pneumonia obsession and that the number of deaths really due to pneumonia is decidedly less—but the results are not yet ready to publish.]

[22] [*Added in Proof.* Results of value all already becoming available. For the four months, April–July, 1924 we have

	Lobar Pneumonia				Broncho Pneumonia			
Cases	361	209	140	69	169	102	63	34
Deaths	186	118	66	49	64	45	32	23
Fatality	52%	57%	47%	70%	38%	45%	50%	70%

The case fatality rates are exceedingly high and the officials are apparently getting very good case reporting. The matter is further discussed by Dr. Vaux in his recent article cited in Note 15.]

steady (witness the sharp trend of the recent past from undefined to lobar pneumonia throughout the registration states). If a radical change of the trend of pneumonia mortality takes place in Pittsburgh in the next few years, its causes must be analyzed impartially if they are to be of value in our true understanding of the ways of this prevalent disease which lays such a heavy hand upon our population, which without warning cuts off so many useful lives, and which with cancer and the organic heart diseases offers such a challenge to the medical and public health professions.

VI. Summary.

21. The death rate from pneumonia (all forms) in the 10 original registration states of 1900 has remained stationary or declined slightly during the period 1900–1920; in the City of Pittsburgh the rate, starting at about the general level, has risen until it is higher than in any other large city. Such a situation calls for a critical study. With the aid of figures from the reports of the U. S. Bureau of Census and additional figures furnished in a most obligingly cooperative spirit by the Health Department of the City of Pittsburgh under Dr. C. J. Vaux, the authors have set forth in detail the course of mortality from pneumonia in Pittsburgh for the 21 years 1900–1920 and in a cursory way for the 50 years 1873–1922; they have also made comparisons with other districts.

On this statistical background are discussed the various suggestions that have been made to account for Pittsburgh's exceptionally high and exceptionally increasing rate, namely, the race and sex distribution of the population, the unhygienic housing, and the severe labor conditions in the city, the importation of labor into the city, the excess of smoke and dust due to the great industrial activity, and the possibility of a tendency to report pneumonia as a cause of death relatively more frequently than is customary elsewhere. It is believed that all these factors probably contribute to Pittsburgh's excess mortality from this disease, but it is difficult to admit that the race or sex distribution of the population are becoming progressively so much more unfavorable and the labor conditions or housing situations are so rapidly worsening as to afford a rational explanation for any considerable part of the increase. It seems highly probable that

smoke and dust may act accumulatively so as to produce not only an excess death rate but an increase in it, continuing possibly even after there has been some improvement in the control of the smoke and dust nuisance. It seems also highly probable that there is a tendency to report pneumonia as a cause of death somewhat more freely than elsewhere; different habits in reporting from time to time and from place to place are not unknown, and indeed are unavoidable.

No satisfactorily sure conclusions can, however, be reached as to the cause of Pittsburgh's excess mortality from pneumonia or its increase without a prolonged carefully controlled scientific study of all phases of the situation, social, statistical, and in particular physical and physiological. The situation is so bad that it is to be hoped that any efforts of the Department of Health of the City of Pittsburgh to deal with this problem will meet cordial support; but until the proportionate influences of the various causes have been ascertained all palliative measures must be tentative and their results critically examined. Insofar as the matter is one of industrial hygiene, as it seems in no small measure to be, it is important to have the active coöperation of the industry in studying the condition and in combatting it, and it is probable that rational efforts of the industry would be repaid not only in civic betterment but in lessened costs of production.

Harvard School of Public Health,
Boston, Mass.,
April, 1924.

INDEX

A

B

C

D

E

F

OBITUARY NOTICES OF MEMBERS DECEASED.

OBITUARY NOTICES OF MEMBERS DECEASED.

WOODROW WILSON.[1]

By JOHN H. FINLEY.

(*Read March 7, 1924.*)

The author of the "History of the People of the United States" states with a touch of satire that when Washington had said farewell to his officers in New York and was on his way to Annapolis to resign his commission as Commander-in-Chief of the Army, The American Philosophical Society at Philadelphia turned from the consideration of learned papers on Improved Methods for Quilling the Harpsichord and Observations on the Torporific Eel to pay homage to the great chief. So, this body, which has the marks of immortality upon it and is not subject to the vicissitudes of mortality, pauses at the passing of another American Commander-in-Chief—one who has been a leader in the greatest war the world has ever known—turns from the consideration of more profound scientific subjects than those of a century and a half ago, to pay homage to one who has resigned his commission into the hands of Deity as a soldier who fought a good fight and kept the faith—Woodrow Wilson, the historian who both wrote and made history.

I was several years ago very unexpectedly called upon to say a word concerning him at a public dinner given to him, when he was Governor of New Jersey. Having but a little time before returned from abroad, I had to say that I had not before met "Governor" Wilson, but I added that I had some acquaintance with President Wilson of Princeton University and that I had known well and had unbounded admiration for Professor Wilson as a teacher and associate. So I find myself, tonight, led to speak of the man, the teacher and the scholar, philosopher and member of this Society, Woodrow Wilson, at whose feet I sat as a student at Johns Hopkins University where he was a lecturer and at whose side I sat as a professor at Princeton University. I am perhaps disqualified by this relationship to speak dispassionately of him, but you will be able

to determine the coefficient of expansion in my estimate and make the allowance for the warmth of my expression. But I should by his own standards be permitted to speak of him even in this way, for as he himself said: "the truth cannot be told by a dispassionate annalist." Moreover, I have his authority for departing from a chronological arrangement, for "the best arrangement," he said, "is always that which displays not the facts themselves, but the subtle and else invisible forces that lurk in the events and in the minds of men." Thus with his own warrant, I begin as did Dante with his master Virgil, "nel mezzo del cammin di nostra vita," for we were at the very midway of life when I came to know him—whom I can fitly characterize as Dante did Virgil, as the "well-spring from which such copious floods of eloquence have issued."

The best picture of him in those days was one that he unconsciously made of himself when describing Walter Bagehot, whom he called a "literary politician" and of whom he said: "You have in him a very superior species of the man thoughtful, full of manly straightforward meaning, full of knowledge and a consuming desire for it, . . . genial withal yet with the geniality of a man of wit and alive in every fibre of him with a life he can communicate to you." Though he had the visage of a Covenanter and a grimness when his face was set with some fixed purpose of his mind, it became the most kindly of countenances when lighted in public discourse or in private conversation with a friend. He could jest (and he was full of jest and joke) "to your instruction and beguile you into being informed beyond your wont and wise beyond your birthright." Young men were seized with a thrill of expectancy when he entered to begin a lecture. His charm was enhanced by the rugged lines of his homely face. I spoke a few nights ago with a young man in his thirties who was a Princeton student in the days when the "Quad" system was being discussed with acrimony. He and his companion, opposed to President Wilson's plan, went to a meeting at which he was to speak, vowing to keep themselves beyond the influence of anything but cold facts. The President made a speech which was devoid of any concrete facts, but the whole audience was lifted to its feet by his simple convincing presentation of the case—so the student testified.

If Woodrow Wilson were asked to justify himself, in omitting the mention of facts—as I once heard him defend an overstatement—he would answer: "The facts do not constitute the truth. The truth is abstract, not concrete. It is the *just* idea, the *right* revelation of what things mean." No inventory of items, he contended, would even represent the truth. They might even obscure it. The true teacher, the true leader, as the true historian "works always for the whole impression." This was what he was saying of the historian, at any rate, back in the nineties, when the students were crowding his courses as the pupils of Abelard flocked to him out in the desert where he built an oratory of stubble and established the School of Paraclete. And was this not the explanation, in some measure at least, of Woodrow Wilson's failure to carry his supreme policy, having to sit down day after day with concrete facts, with an inventory of items which obscured by their number and complexity the great truth he was trying to express concerning the relationship of nations in the salvation of the earth?

Back in his library, he wrote before the days of his political activity could even have been dreamed of: "The ordinary literary man even though he be an eminent historian is ill enough fitted to be a mentor in affairs of government." "For," he added, "it must be admitted, things are for the most part very simple in books and in practical life, very complex. Not all the bindings of a library inclose the various world of circumstance." Nevertheless, it was to such a man that the practical politician did go, but fetched him away from his library into the forum where "politics gained a profound expounder who was no politician." And the literary man had to put away some of the things that were so simple in books. The author of "The State" and the historian of the United States, the writer of "mere literature" and the critic of "congressional Government" found himself the Chief Magistrate of the greatest of States and in the world and for a time as Louis XIV, the State itself, and the virtual dictator of the Congress of which he had written in his learned doctor's thesis. No one ever came better prepared in a knowledge of political theory into the Presidency. He was so great a master of his subject that he could take liberties with it. He could, to borrow his own language, "slap

his propositions on the back and be hail fellow well met with them." But with all his obvious effort, he could not be hail fellow well met with the people themselves, though no one could converse with more charm.

If I were to imitate Plutarch and try to bring out the distinctive character of one by comparison with another, as Aristides with Marcus Cato, I should most naturally speak of Grover Cleveland and Woodrow Wilson, who by a singular fortuity were both near to me, though not near to each other. Going to Princeton one night in 1899 as a journalist to hear Mr. Cleveland lecture on the Independence of the Executive, I met my former teacher, Professor Wilson who had long before introduced me to Mr. Cleveland, was told by him of a new chair of politics, the first in America, that had been endowed and was asked whether I was willing to accept a nomination as its first encumbent. I had thus the opportunity in the few years that followed to know these two great men: Mr. Cleveland in his very fiber a democrat, believing the people, instinctively sympathetic with them, deriving his doctrines not from books but from his contacts with men, little acquainted with books—a little suspicious of words; and Mr. Wilson as complete a democrat in his philosophy. He was intellectually a devout disciple of democracy. But he learned it by the tuitions of his mind and not of his heart. And he had one of the greatest of minds to teach him. He once said: "His mind is a great comfort to every man who has one." In his own, he had supreme comfort and complete confidence. It equipped him of its tuitions by the hard crystallization of years of study to instruct the mind of the world and enabled him to stand "as a distinct and imperative individual among those who express the world's thought." As an expounder of government, he sought and found his materials far and wide, "standing with the poets as well as lawgivers." "There is more of a nation's politics to be got out of its poetry than out of all its systematic writers upon public affairs and constitutions. Epics are better mirrors of manners than chronicles; dramas oftentimes let you into the secrets of statutes; orations stirred by a deep energy of emotion or resolution, passionate pamphlets that survive their mission because of the direct action of their style along permanent lines of thought, contain more history than parliamentary pamphlets."

He contended, in the face of the scientist and his study of form and structure, that it was not knowledge that moved the world, but ideals, convictions, the opinions or fancies which have been held or followed. So, though he studied the anatomy of government and knew it as thoroughly as any doctor of science studies and knows the human frame, which he himself in his dying words likened to a piece of machinery, he was ever practicing the "vivisection of reading literature" to acquaint himself with the spirit that made use of the machinery of government. But he never knew the rough, narrow-ranged comradeship of those who are at the foundations of democracy, on whose minds and hearts its safety in the world depends. He was *in* the world and the most potent man in it for his day, but in a sense he was not *of* it.

But it was this very detachment—the meditation apart—which made him as a voice of one coming out of the wilderness and crying across the earth, in every corner of which he was heard. Yet he realized as he said in his address at Lincoln's birthplace, that the hopes of mankind cannot be kept alive by words merely, that they must be transmuted into the life and action of society; but after all they must first have a voice. In the beginning is always the word.

It was Wilson's words that after all got things done in the war. Many of us were impatient with his "notes" and there were some of these notes that would better never have been written. But it was ultimately by his messages, crowned with this immortal quality of ecstasy, that his own people were assembled into the defense of world freedom and the nations that sat in darkness brought to see "a great light" as in his Flag Day Address in 1917:

"For us there is but one choice. We have made it. Woe be to the man or group of men that seeks to stand in our way in this day of high resolution, when every principle we hold dearest is to be vindicated and made secure for the salvation of the nation. We are ready to plead at the bar of history and our flag shall wear a new lustre. Once more we shall make good, with our lives and fortunes, the great faith to which we were born and a new glory shall shine in the face of the people."

The spirit that had become flesh, again became spirit in the words that again and again stirred people to an exaltation, such as in turn exalted the whole nation.

Jefferson had written on his tomb an inscription telling not what the people had done for him, but what he had done for the people. There is no mention made of his having been Governor of Virginia, Secretary of State, Vice-President or President of the United States. What he seemed to wish remembered was that he was, as it is written, the author of words—author of the Declaration of Independence and of the Statute of Virginia for Religious Liberty and the Father of the University of Virginia. Two of his three great contributions were of words merely, but they were the precursors of independent national life and freedom of worship. Woodrow Wilson will be remembered long after his name becomes but one of a hundred or hundreds in the lengthening list of Presidents' names, as the author of words upon which the nations hung—words that caused the millions to pause in their fighting and which became the basis of the parley of peace—words which framed a covenant for the "enduring" peace of the world. They are only words, but they carry an exalted hope of humanity and must some day find their guerdon in deeds.

He wrote a Calendar of Great Americans back in those days, when he was a historian and teacher and essayist. He excluded from this list several great men born and bred in America who were not after all Americans: Hamilton and Madison whom he regarded as great Englishmen born in the New World, John Adams and Calhoun great provincials, Asa Gray and Emerson "who might have been native to any clime" and Jefferson and Benton who were of mixed breed, one permeated by a strain of French philosophy and the other a "pompous antinomy." Marshall and Webster were, however, distinctly American, viewing the fundamental law as a great "organic product" as well as a charter of authority. But he put first as a sort of multiple American the founder of this Society, Benjamin Franklin, a man possessed of the American speech and with an "unmistakeable touch of greatness and distinction." Then he named Washington who hardly seems an American as most of his biographers depict him, who was, however, as thoroughly American as Jackson who came into our national politics "like a cyclone from off the Western prairies," or Lincoln the "supreme American of our history." But when Professor Wilson wrote this calendar (which

included Clay, Grant and Lee, Sam Houston and Patrick Henry and in letters Lowell and Curtis, but no one in science) and prophesied that many types of Americanism would rise out of its multiform life, he could not have suspected that his own name would ever be in the political list of great Americans even if he dared to hope that some day that name might be put with those of Lowell and Curtis in letters or among the great historians with Greene and Gibbon and Carlyle and Macaulay (at all of whose doors he knocked when in search of a master), or even with the writers of mere literature.

This passage from his own writing not only shows how intimately acquainted he was with varied immortal forms, but also illustrates with what rich fabrics he could clothe his thought on occasion, when at other times, with like art, he used the simple stone:

"Who shall say how much of Burke's splendid and impressive imagery is part and stuff of his thought or tell why even that part of Newman's prose which is devoid of ornament, stripped of its shining skin and running bare and lithe and athletic to carry its tidings to men, should promise to enjoy as certain an immortality? Why should Lamb go so quaintly and elaborately to work upon his critical essays, taking care to perfume every sentence, if possible, with the fine savor of an old phrase, if the same business could be as effectively done in the plain and even cadences of Mr. Matthew Arnold's prose? Why should Gibbon be so formal, so stately, so elaborate when he had before his eyes the example of great Tacitus, whose direct sententious style had outlived by so many hundred years the very language in which he wrote?"

And yet he was always depicting himself, in a semi-political service, as I have intimated, in what he has written of others, notably Bagehot and Burke back in the nineties or late eighties: "Occasionally," he said, "a man is born into the world whose mission is to clarify the thought of his generation and to vivify it, to give it speed where it is slow, vision where it is blind, balance where it is out of poise, saving humor where it is dry. Such a man," he added, "was Walter Bagehot." "Such a man," with only the slightest change of phrase we should say, "was Woodrow Wilson." For a day in the world's history, the thought of this generation was clarified by him. For a day he vivified the souls of men. For a

day there was a vision glorious before the eyes of the nations. And if he seemed not to give speed by his "watchful waiting," we may recall the words of Mr. Choate back in April 1917:

"Some of us thought the watchful waiting would never cease. But now we see what the President was waiting for and how wisely he waited. He was waiting to see how fast and how far the American people would keep pace with him and stand up to any action he proposed."

It is not for me to appraise his political achievement. His place among outstanding Presidents is beyond challenge. He became a member here as a teacher, a scholar, a political philosopher, a historian and it is as a teacher, a scholar and a political philosopher and historian that I think he would wish to be remembered here. If he had not come into the Presidency at a most critical time in the world's history, he would yet have had rightful distinction as a man of letters. His fellows in letters had long before he became President elected him to membership in the American Academy of Arts and Letters, as you into this eminent society of philosophers. His art had prepared him against oblivion, even though it could not have given him as a mere men of letters a world audience. He had lived, as he said it behooved all minor authors to live, conscious of the risk of being discovered some day. He had already been discovered and had come into the small major group of his day in letters before he came into the still smaller major group of the world's statesmen.

If the touchstone of what an English essayist calls "fine" literature and what Mr. Wilson called "mere" literature or "essential" literature be the presence of "ecstasy"—the voice which tells us distinctly as Mr. Wilson does, that man is "not the creature of the drawing room or the stock exchange, but a lonely, awful soul confronted by the source of All Souls"—then does Woodrow Wilson become a real man of letters. He came in his prime as if born into the world for the very mission, as a teacher of citizenship in the world which humanity has made.

For he was first and last a teacher. His officers were incidental to his task; they were his rostrum. He inaugurated the preceptorial system at Princeton, but he was not a preceptor, sitting down by the side of the individual like Mark Hopkins. He taught the multi-

tudes. His chair as that of Abelard was an "oratory." He spoke as one having authority from a platform, a little above those whose minds he would inflame. This was his method in political as in academic life. He instructed the many by letter and by speech out of the loneliness of his own thinking. He came finally to lecture to the peoples of the earth on planetary jurisprudence, from the greatest chair of international relations ever established in our universe. As Benjamin Franklin at the end of his varied high public occupations wrote himself down in his last will and testament, "Benjamin Franklin, Printer," so the best epitaph for Woodrow Wilson though he had held the highest political office in the United States and the foremost place for a time in the sight of the nations, would be, "Woodrow Wilson, Teacher."

"And so" (to quote Pericles) "with slight change he has received the grandest of all sepulchres." Not that where I saw his body laid, but "a home in the minds of men, where his glory remains fresh to stir to speech or to action as the occasion comes by. For the sepulchre of famous men is the whole earth; and their story is not graven on stone over their native earth, but lives on far away, without visible symbol, woven into the stuff of other men's lives."

MINUTES.

MINUTES.

Stated Meeting, January 4, 1924.

WILLIAM B. SCOTT, Sc.D., LL.D., President, in the Chair.

Dr. Rodney H. True, a newly elected member, subscribed the Laws and was admitted into the Society.

The decease was announced of Dr. Samuel P. Sadtler, Ph.D., LL.D., at Philadelphia, on December 20, 1923, æt. 76.

Dr. John A. Miller, Director of the Sproul Observatory, Swarthmore College, read a paper entitled "The Solar Eclipse of 1923, as seen at Yerbanis, Mexico, by the Sproul Observatory Expedition," illustrated by lantern slides, which was discussed by Prof. Snyder.

The dates of the General Meeting of 1924 were fixed for April 24, 25, and 26.

Stated Meeting, February 1, 1924.

WILLIAM B. SCOTT, Sc.D., LL.D., President, in the Chair.

The Chairman of the Library Committee called special attention to a valuable donation by Mr. William Smith Mason, of Evanston, Illinois, of 64 volumes and 4 pamphlets of Frankliniana, for which the Society voted a resolution of appreciation.

The decease was announced of Basil Lanneau Gildersleeve, LL.D., Litt.D., at Baltimore, Maryland, on January 9, 1924, æt. 92.

Dr. Allen J. Smith, Professor of Pathology at the University of Pennsylvania, read a paper on "The Development of Insulin in the Treatment of Diabetes," illustrated by lantern slides, which was discussed by Doctors Keen, Goodspeed, Smith, and Keller.

Stated Meeting, March 7, 1924.

WILLIAM B. SCOTT, Sc.D., LL.D., President, in the Chair.

Dr. John H. Finley, a recently elected member, subscribed the Laws and was admitted into the Society.

A bas-relief in bronze of President Wilson, a recent purchase of the Curator, was presented.

The decease was announced of

Harry F. Keller, B.S., Ph.D., Sc.D., at Philadelphia, on February 5, 1924, æt. 62.

Jacques Loeb, M.D., Ph.D., Sc.D., at Hamilton, Bermuda, on February 11, 1924, æt. 65.

Hon. Woodrow Wilson, at Washington, D. C., on February 3, 1924, æt. 68.

Dr. John H. Finley, Associate Editor of the *New York Times,* and formerly Professor of Politics at Princeton University, presented a biographical notice of Ex-President Wilson, a deceased member of this Society; after which Mr. Roland S. Morris, Ambassador to Japan during the Administration of President Wilson, made some remarks on the former President.

Stated General Meeting, April 24, 25, 26, 1924.

Thursday Afternoon, April 24.

Opening Session, 2 o'clock.

HAMPTON L. CARSON, LL.D., Vice-President, in the Chair.

Prof. David L. Webster, a newly elected member, subscribed the Laws and was admitted into the Society.

The death was announced of Prince Roland Bonaparte, at Paris, France, on April 14, 1924, æt. 66.

The following papers were read:

"The Fate of the Soul of the Elect in Manichaeism," by A. V. Williams Jackson, A.M., L.H.D., LL.D., Professor of Indo-Iranian Languages, Columbia University. (By title.)

"The Bornholm Dialect of Danish," by John Dyneley Prince, B.A., Ph.D., Envoy Extraordinary and Minister Plenipotentiary to Denmark. (By title.)

"Balder and the Golden Age," by Hermann Collitz, A.M., Ph.D., Professor of Germanic Philology, Johns Hopkins University.

"Some Effects of Baths on Man," by H. C. Bazett, B.Ch.

(Oxon.), F.R.C.S., M.D., Professor of Physiology, University of Pennsylvania. (Introduced by Dr. A. C. Abbott.)

"Differential Permeability and Cell Reaction," by M. H. Jacobs, Ph.D., Professor of General Physiology, University of Pennsylvania. (Introduced by Dr. A. C. Abbott.)

"Pneumonia in Pittsburgh," by Ewald Tomanek, M.D., and Edwin B. Wilson, A.B., Ph.D., Harvard School of Public Health.

"The Amending Provision of the Federal Constitution in Practice," by Herman V. Ames, A.M., Ph.D., Professor of History, University of Pennsylvania.

"On the Authorship of the Anonymous Pamphlet Published in London, in 1760, Entitled 'The Interest of Great Britain Considered with Regard to Her Colonies,'" by I. Minis Hays, A.M., M.D., of Philadelphia.

"The Nation's Transportation Problem," by Emory R. Johnson, Litt.M., Ph.D., Sc.D., Professor of Transportation, University of Pennsylvania, which was discussed by Mr. Carson.

"Obstacles to International Commerce," by Lewis M. Haupt, Sc.D., LL.D., of Philadelphia.

"The Scientist and an International Language," by Roland G. Kent, A.M., Ph.D., Professor of Comparative Philology, University of Pennsylvania. (Introduced by Dr. McDaniel.)

Friday, April 25.

Executive Session, 10 o'clock.

William B. Scott, Sc.D., LL.D., President, in the Chair.

The President delivered his annual address.

The Proceedings of the Council were submitted, with the recommendation of fifteen nominees for election this year.

The Reports of the Treasurer, of the Audit Committee, and of the Finance Committee were presented, with the latter's recommendation of appropriations for the year 1925, which were unanimously voted.

The following proposed amendments to the Laws, having been favorably reported by the Council, were unanimously adopted:

To Chapter XI, Section 8, add:

"In the selection of nominees for presiding Officers and Councillors the several departments of science shall be represented so far as practicable, and due consideration shall be given to geographical location so as to preserve the national character of the Society."

In Chapter XI insert the following new sections:

"All standing Committees shall hold stated meetings at least three times in the year."

"All Boards of Officers and Committees shall keep regular minutes of their proceedings which shall be in the custody and care of the Assistant Secretary."

"No indebtedness shall be incurred for which there has not been an appropriation previously made; nor shall any indebtedness be incurred or bill approved for payment except by a duly recorded vote of the committee concerned."

Morning Session, 10:30 o'clock.

WILLIAM B. SCOTT, Sc.D., LL.D., President, in the Chair.

The following recently elected members subscribed the Laws and were admitted into the Society:

T. Wayland Vaughan
W. J. Sinclair
Charles E. Allen
S. A. Mitchell and
Charles D. Hazen

The following papers were read:

"Inheritance by Tetrad Sibs in *Sphaerocarpos*," by Charles E. Allen, Ph.D., Professor of Botany, Columbia University.

"The Behavior of *Oenothera neo-Lamarckiana* in Selfed Line through Six Generations," by Bradley M. Davis, A.M., Ph.D., Professor of Botany, University of Michigan, which was discussed by Professors Shull, Davis, and MacDougal.

"Types and Variants in Certain Coenobic Plants," by Robert A. Harper, M.A., Ph.D., Professor of Botany, Columbia University.

"A Second Independently Inherited Factor in the Evening Primroses (*Oenothera*)," by George H. Shull, B.S., Ph.D., Professor of Botany and Genetics, Princeton University, which was discussed by Professors Scott, Davis, and Shull.

"Arrangement and Action of Material in the Plasmatic Layers and Cell Walls of Plants," by D. T. MacDougal, M.A., Ph.D., LL.D., Director of Dept. of Botanical Research, Carnegie Institution, which was discussed by Professors Scott and Shull.

"The Curve of Population Growth," by Raymond Pearl, Ph.D., Sc.D., LL.D., Professor of Biometry and Vital Statistics, School of Hygiene and Public Health, Johns Hopkins University, which was discussed by Professors Brown, Pearl, L. M. Haupt, Kennelly, and Hobbs.

"Faunal Life Zones of Mongolia—Jurassic to Upper Pliocene," by Mr. Walter Granger, Palaeontologist of the Third Asiatic Expedition, of American Museum of Natural History.

"Fauna of the Santa Cruz Beds of Patagonia," by William B. Scott, Ph.D., Sc.D., LL.D., Professor of Geology, Princeton University.

"Fauna of the Concretionary Zone of the Oreodon Beds of the White River Oligocene," by W. J. Sinclair, Ph.D., Assistant Professor of Geology, Princeton University.

Afternoon Session, 2 o'clock.

William B. Scott, Sc.D., LL.D., President, in the Chair.

A bronze memorial tablet of Mr. Henry LaBarre Jayne, who was for many years Treasurer of the Society, was placed on deposit here by the Jayne Memorial Committee. Mr. Roland S. Morris, in making the presentation, spoke as follows:

We have all known at least once in our lives persons whose spiritual qualities give us a present sense of their immortality. They associate with us in the daily tasks of this material world; they share with singular sympathy and insight the achievements and frustrations which is the experience of all of us and yet to those who are privileged to know them they convey a feeling of strength and permanence which is certain to persist beyond a world of changing appearance. The passing of such vivid personalities beyond our material vision only serves to emphasize in us the conviction of their continued spiritual existence. In the ancient words of the Latin liturgies we feel that they are "born into the better life," of which their life on earth was but a promise and a preparation. Henry LaBarre Jayne was such a personality. He loved this world, its beauty, its perilous possibilities; its baffling mysteries. This love found expression in a lifelong interest in every form of art which sought truthfully to mould in

imperishable forms the elusive spirit of beauty, the tragedy and comedy of human experience or the aspiration of the souls of man.

He loved the riches of the mind and always thought of himself with characteristic modesty as a private enlisted for life in the adventurous search for truth. Above all he loved his fellow-man with an almost religious passion and no human soul was an alien to his broad and generous sympathies.

But it was his truly spiritual insight which gave to these affections and to the personality which unified them that sense of his continued presence which we feel today.

How natural—how inevitable—that when he left us in the body we, his friends, should seek to find some material expression of this continued presence in our lives. Thus spontaneously a group of his fellow-citizens, representative of the varied interests of an active life organized the JAYNE MEMORIAL FUND to hold, if I may so express it, something of his spirit for the generations to come, who knew him not in the flesh.

Out of this movement grew the Jayne Memorial Lectures—a permanent foundation adequately endowed, designed to give each year to some leader of thought an opportunity to present to the world the results of his research. We hope that this foundation may ever retain that spirit of tolerance, liberty and deep sincerity which is associated with our memory of him whose name it bears. In this way alone can it make any worthy contribution to American thought. But we, his friends, his associates in various activities, longed for some memorial of him more personal than the foundation could be. So we turned to a great artist and asked him to express in bronze our memory of his ever youthful vigor and enthusiasm.

It is fitting, Mr. President, that the Trustees of the University Extension Society should place in the custody of the American Philosophical Society this memorial of our friend. To the University Extension Society HENRY LABARRE JAYNE gave unsparingly of his time and strength. To the American Philosophical Society with its fine traditions of truth and its achievements in the field of scientific research he gave affectionately the riches of his creative personality. We leave in your keeping this beautiful reminder of him amid the memorials of the great minds he loved and reverenced. In our hearts we retain the inspiration of his unselfish life, of his immortal spirit.

"A great citizen who gave generously of his time, his talents, and his fortune fortune to the diffusion of knowledge; the progress of the fine arts; the development of the drama and the promotion of civic ideals."

The Symposium followed, the subject being "Are the Various Races of Man Potentially Equal?" The speakers were:

Franz Boas, Ph.D., LL.D., Sc.D., Professor of Anthropology, Columbia University.

H. U. Hall, Curator General Ethnology, University Museum, University of Pennsylvania. (Introduced by Dr. Gordon.)

Alexander Goldenweiser, Ph.D., Professor of Anthropology,

New School for Social Research, New York City. (Introduced by Dr. Gordon.)

There was discussion by Professors Scott, Conklin, Gordon, Boas, and Kennelly.

The Friday evening Lecture was given by Dr. Dayton C. Miller, Professor of Physics, Case School of Applied Science, Cleveland, who spoke on "Visible Sound," with experimental illustrations.

Saturday, April 26.

Executive Session, 10 o'clock.

William B. Scott, Sc.D., LL.D., President, in the Chair.

Pending nominations for Officers and Members were read and the Society proceeded to an election. The Tellers subsequently reported that the following had been elected:

President.

William B. Scott.

Vice-Presidents.

Hampton L. Carson,
Henry Fairfield Osborn,
W. W. Campbell.

Secretaries.

Arthur W. Goodspeed,
John A. Miller.

Curator.

William P. Wilson.

Treasurer.

Eli Kirk Price.

Councillors.

(To serve for 3 years.)

Henry Crew,
S. W. Stratton,
W. E. Lingelbach,
V. C. Vaughan.

Members.

Charles McLean Andrews, A.M., Ph.D., L.H.D., New Haven.
James Roland Angell, A.M., Litt.D., LL.D., New Haven.
Harry Bateman, M.A., Ph.D., Pasadena.
Albert Francis Blakeslee, A.M., Ph.D., Cold Spring Harbor.
John William Davis, A.B., LL.B., LL.D., New York.
Gano Dunn, M.S., E.E., New York.
Livingston Farrand, A.M., Ph.D., Ithaca, N. Y.
Charles A. Kofoid, A.M., Ph.D., Berkeley, Calif.
Armin O. Leuschner, A.B., Ph.D., Sc.D., Berkeley, Calif.
Frank Leverett, B.Sc., Ann Arbor, Mich.
Graham Lusk, A.M., Ph.D., Sc.D., New York.
Charles E. Mendenhall, B.S., Ph.D., Madison, Wis.
Charles R. Stockard, M.S., Ph.D., New York.
William Sydney Thayer, M.D., Baltimore.
Clark Wissler, A.M., Ph.D., New York.

Morning Session, 10:30 o'clock.

WILLIAM B. SCOTT, Sc.D., LL.D., President, in the Chair.

The following papers were read:

"The Effects of Temperature on the Rate of Embryonic Development of Certain Orthoptera," by J. H. Bodine, A.B., Ph.D., Instructor in Zoölogy, University of Pennsylvania. (Introduced by Dr. McClung.) Discussed by Professors Parker, Davis, and Dahlgren.

"Symbiotic Luminous Bacteria as Used by Fishes," by Ulric Dahlgren, A.B., M.S., Professor of Biology, Princeton University.

"Transplantations of the Spinal Cord," by Samuel R. Detwiler, Ph.D., Assistant Professor of Zoölogy, Harvard University. (Introduced by Dr. Donaldson.)

"The Fishes Used Against Yellow Fever in Colombia," by Carl H. Eigenmann, A.M., Ph.D., Professor of Zoölogy, Indiana University.

"The Amount of Carbon Dioxide Excreted by One Centimeter of Frog Nerve Fiber," by George H. Parker, Sc.D., Director of Museum of Comparative Zoölogy, Harvard University.

"Sex in the Right and Left Sides of the Bird's Body," by Oscar Riddle, Research Staff, Carnegie Station for Experimental Evolution, Cold Spring Harbor. (Introduced by Dr. Eigenmann.)

"The Prediction of the Basal Metabolism of Girls," by Francis G. Benedict, Ph.D., Sc.D., Director, Nutrition Laboratory of the Carnegie Institution.

"Some Phases of the Life of Gambetta," by Charles Downer Hazen, Ph.D., L.H.D., Litt.D., Professor of History, Columbia University. Discussed by Prof. Scott.

"Vicarious Atonement," by Paul Haupt, A.M., Ph.D., LL.D., Professor of Semitic Languages, Johns Hopkins University. Discussed by Professors Scott and Haupt.

Afternoon Session, 2 o'clock.

William W. Campbell, M.S., Sc.D., LL.D., Vice-President, in the Chair.

The following papers were read:

"The Sonic Depth Finder," by Harvey C. Hayes, Ph.D., U. S. Naval Experiment Station, Annapolis. (Introduced by Mr. Bryant.) Discussed by Professors Haupt and Hayes.

"Some New Experiments in Gravitation" (Fourth Paper), by Charles F. Brush, Ph.D., Sc.D., LL.D., of Cleveland.

"Further Results Concerning the Earth's Magnetic and Electric Fields," by Louis A. Bauer, C.E., M.S., M.A., Ph.D., Director of the Department of Terrestrial Magnetism, Carnegie Institution.

"Abnormal Under-voltage Arcs in Gases," by C. B. Bazzoni, A.M., Ph.D., Professor of Experimental Physics, and Mr. J. T. Lay, Research Associate, University of Pennsylvania. (Introduced by Professor Goodspeed.) This was discussed by Mr. Smyth.

"Application of Positive Ray Analysis to Problems of Ionization," by H. D. Smyth, Ph.D., Research Fellow, Princeton University. (Introduced by Professor Karl T. Compton.)

"Some Properties of Simple Electric Conducting Net Works," by A. E. Kennelly, A.M., Sc.D., Professor of Electrical Engineering, Harvard University.

"Wave Lengths of Iron Lines in the Vacuum Arc." (By title.)

"On the Shift of the Solar Lines Predicted by the Theory of Relativity," by Keivin Burns, Ph.D., Astronomer, Allegheny Observatory. (Introduced by Dr. H. D. Curtis.) Discussed by Professors St. John, Kennelly, and Burns.

"On the Light Deflections in the Sun's Gravitational Field," by Robert J. Trumpler, Ph.D., Assistant Astronomer at Lick Observatory. (Introduced by Dr. W. W. Campbell.) In Dr. Trumpler's absence Dr. Campbell presented the paper, which was discussed by Professors Shapley and Eisenhart.

"Exploring the Solar Atmosphere," by Charles E. St. John, Ph.D., Astronomer, Mt. Wilson Observatory. (Introduced by Dr. John A. Miller.) Discussed by Dr. Burns.

"The Present Periodic Table of the Atoms," by Monroe B. Snyder, Director Emeritus of the Philadelphia Observatory.

Special Meeting, May 2, 1924.

WILLIAM B. SCOTT, Sc.D., LL.D., President, in the Chair.

Pursuant to the call of the President a Special Meeting was held for the award of the John Scott Medals and Premiums by the City of Philadelphia through its Board of City Trustees.

Acceptances of Membership were read from

Mr. Gano Dunn, of New York.

Dr. Livingston Farrand, of Ithaca, N. Y.

Mr. Frank Leverett, of Ann Arbor, Mich.

Dr. William S. Thayer, of Baltimore.

The decease was announced of Ernest Fox Nichols, D.Sc., at Washington, D. C., on April 29, 1924, æt. 55.

The award of the John Scott Medals and Premiums was made to the following:

Frederick G. Banting, M.D., Physiologist, of London, Ontario, who in 1920 and 1921 succeeded in preparing a potent extract of the experimentally atrophied pancreas; a condition produced by ligating the pancreatic duct. The injected extract increased materially the life of depancreatized dogs by enabling them to retain larger amounts of sugar. In these researches there were associated with Dr. Banting, Doctors J. J. R. McLeod, Best, and Callip.

William W. Coblentz, Ph.D., Physicist of the U. S. Bureau of Standards, for his unsurpassed skill in the design and construction of thermopiles and radiometers of the highest sensitivity with which he has actually measured the radiation of the fainter stars.

Elmer Verner McCollum, Ph.D., Sc.D., Professor of Biochemistry, School of Hygiene and Public Health, Johns Hopkins University, who demonstrated in 1913 a growth promoting vitamin in butter fat, the first of a long series of researches by him and his collaborators on the presence in various foods of other similar substances, promoting growth and maintaining health.

Ralph Modjeski, D.Eng., of New York City, for his skill in bridge designing, having designed and built the Columbia and Williamette River bridges in Oregon, the McKinley Bridge at St. Louis, the Broadway Bridge at Portland, Oregon, the Cherry Street Bridge at Toledo. He is now Chief Engineer of the Delaware River Bridge.

Stated Meeting, November 7, 1924.

William B. Scott, Sc.D., LL.D., President, in the Chair.

Letters accepting membership were received from:

Dr. Charles McLean Andrews, of New Haven.

President James Roland Angell, New Haven.
Prof. Harry Bateman, of Pasadena.
Dr. Albert F. Blakeslee, of Cold Spring Harbor, L. I.
Hon. John W. Davis, Esq., of New York.
Prof. Charles A. Kofoid, of Berkeley, Calif.
Prof. Armin O. Leuschner, of Berkeley, Calif.
Prof. Charles E. Mendenhall, of Madison, Wis.
Dr. Graham Lusk, of New York.
Dr. Charles R. Stockard, of New York.
Dr. Clark Wissler, of New York.

The decease was announced of the following members:

Baron d'Estournelles de Constant, at Paris, France, May, 1924.
Dr. Robert S. Woodward, at Washington, June 29, 1924, æt. 74.
Dr. Robert G. LeConte, at Philadelphia, August 6, 1924, æt. 59.
Dr. George Arthur Piersol, at Philadelphia, August, 7, 1924, æt. 68.
Prof. John J. Stevenson, at New Canaan, Conn., August 10, 1924, æt. 82.
Dr. Henry Kraemer, at Mt. Clements, Mich., September 9, 1924, æt. 56.

Prof. W. R. Newbold of the University of Pennsylvania read a paper on "The Eagle and the Basket on the Chalice of Antioch" which was illustrated by lantern slides, and discussed by the President.

Stated Meeting December 5, 1924.

No quorum present.

Special Meeting, December 10, 1924.

William B. Scott, Sc.D., LL.D., President in the Chair.

Dr. William S. Thayer, a newly elected member subscribed the Laws and was admitted into the Society.

Prof. A. S. Eddington, F.R.S., of Cambridge, introduced by Prof. J. A. Miller, read a paper on "The Interior of Stars." Discussed by the President, Professor Russell and Dr. Miller.

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